Natural Science

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NOTES AND COMMENTS.

Eliminated.

IT is one of the conditions of continued vigorous activity on an organism's part that income be at least equal to expenditure, and the same is true of journals. To try to sustain the activity when the aforesaid condition is not fulfilled is not uninteresting, but there are limits to the possibility of continuing it. We regret to say that we have reached these limits as regards *Natural Science*, of which this is the last number, so far as we are concerned. In spite of generous support from many during the past year, and our own endeavours in publishing and editing, the journal has not reached that measure of success which would seem to us to warrant another year's experiment. We make our bow, then, to the process of natural elimination.

Nature Studies.

THERE has been much talk of late concerning nature-studies and their more forcible introduction as part of school-education. On the one hand we hear the conservatism of those who think that education had much better continue "on the old lines," that is, without any regulated instruction regarding our natural environment except in so far as that means man and his many inventions. The proper study of mankind, they say, is man, forgetting that he does not live *in vacuo*, and is really unintelligible apart from his non-human environment. On the other hand we hear the enthusiasm of those who think that there is a new panacea for the ills of minds and morals in a codified system of scientific teaching. To any one who is acquainted with the rudiments of the rapidly advancing art of paedagogics or possessed of unbiassed common-sense, the two extreme positions seem absurd, the practical problem being to work our way towards a teaching of the humanities which will be scientific, and a learning of science which will be humanitarian.

Those who are seriously interested in the question would find food for reflection if they would take opportunity to become acquainted with "Nature Studies in Berkshire," by John Coleman Adams (New York and London: G. P. Putnam's Sons, pp. 225, 1899). It is not that there is any new discovery in the book; it is the rediscovery of delight. It will probably not even instruct, but it may possibly enlighten. It is not an educational compendium; it is a work of art. In a beautifully bound and printed volume, with fine photogravures, and in a style which sometimes reminds one of Burroughs, the author tells us of the American Berkshire; and the titles of some of the chapters will suggest his happy mood: A Whisper from the Pines, The Seamy Side of Summer, At the Sign of the Beautiful Star, The Great Cloud Drive, The Fruitage of Beauty. He excels himself perhaps in "The Circumvention of Greylock," which means "a bicycle run round a hill," but the difference between his title and ours is the difference between light and darkness. We have referred to the book here because of our conviction that its value lies in its being an expression of delight in nature by a cultured gentleman, and that if "nature study" does not at least lead towards this, it is not likely to mean more than another millstone about the neck of youth.

The Production of Parthenogenesis in a Sea-Urchin.

It is not long since Delage made a remarkable experiment, which seemed to prove that the nucleus and centrosome of the ovum were not essential to reproduction. Now comes Professor Loeb of Chicago, and, likewise by actual experiment, makes out that even the spermatozoon is not necessary. His results are given in a short note "On the nature of the process of fertilisation and the artificial production of normal larvae (plutei) from the unfertilised eggs of the sea-urchin" (*Amer. Journ. Physiol.* vol. iii. pp. 135-138, Oct. 1899). As the outcome of a long series of experiments and inductions, he was led to believe that the only reason why the eggs of marine animals did not develop parthenogenetically was that something in the constitution of sea-water prevented it. That something, he inferred from experiments on the contraction of muscles, was the presence or absence of ions of sodium, calcium, potassium, and magnesium. The two former require to be reduced, the two latter to be increased: "a great number of variations in this sense might bring about the desired effect." Without going into details, Professor Loeb states briefly that "the mixture of about 50 per cent $\frac{10}{8}n$ $MgCl_2$ with about 50 per cent of

sea-water[1] was able to bring about the same effect as the entrance of a spermatozoon. The unfertilised eggs [of the sea-urchin *Arbacia*] were left in such a solution for about two hours. When brought back into normal sea-water they began to segment and form blastulae, gastrulae, and plutei, which were normal in every respect. The only difference was that fewer eggs developed, and that their development was slower than in the case of the normal development of fertilised eggs. With each experiment a series of control experiments was made to guard against the possible presence of spermatozoa in the sea-water." Professor Loeb's conclusion is "that the unfertilised egg of the sea-urchin contains all the essential elements for the production of a perfect pluteus." "All the spermatozoon *needs* to carry into the egg for the process of fertilisation are ions to supplement the lack of" favourable ions, "or to counteract the effects of the other class of ions in the sea-water, or both. The spermatozoon *may*, however, carry in addition a number of enzymes or other material. The ions and not the nucleins in the spermatozoon are essential to the process of fertilisation." Professor Loeb believes that the same principles hold good for the fertilisation of other, if not all, marine animals, although the ions involved will probably differ in various species. By marine animals he seems to mean those whose eggs are deposited before fertilisation. At all events he does not include mammals, in which class he considers it possible that parthenogenesis is prevented only by the ions of the blood, and that a transitory change in those might allow of it.

The experiments and conclusions of Loeb are consistent with those of Delage, Ziegler, Norman, Driesch, and others. All the ideas as to the extreme importance of nucleus, and centrosome, and polar bodies and the like, are being much shaken, and it seems as if the ground were being cleared for an entirely new and far less complicated theory of sexual reproduction and heredity. It would be interesting to combine the experiments of Delage and Loeb, and to see if an ovum could be made to develop without either its own nucleins or those of the spermatozoon.

The Record of a Great Work.

In four thick volumes the famous chemist Berthelot has told the story of his work at the "Station de Chimie végétale de Meudon" from 1883 to 1899 ("Chimie végétale et agricole." Paris: Masson et Cie. 1899). The first volume deals with the experiments bearing upon the fixation of nitrogen by micro-organisms in the soil or associated with the roots of Leguminosae, by silent electrical discharges in the air, and by other means. In the second volume the central subject is the

[1] These numbers are according to corrections made by the author in a reprint kindly sent by him.

chemical history of an annual plant from germination to death, in connection with which the author recognises the enthusiastic work of his colleague, Mr. G. André. The third volume consists of special researches on the chemistry of plants, the distribution of particular elements, the alleged formation and distribution of nitrates in plants, the formation of oxalic acid and carbonates, the process of respiration, and so on. The fourth volume has mainly to do with the soil, the chemical nature of humus, and the physiological value of the various mineral substances. It concludes with an account of the author's numerous researches on the chemistry of wine. Many of the illustrious chemist's results are familiar through previous publication, and have been met with no small amount of criticism; it is all the more important that we should now have them in collected form and in detailed expression, which enables us to see more clearly the unequal strength of the evidence on which the several conclusions rest. As the record of a great work persistently prosecuted for many years and justified by many results of practical and theoretical importance, the book must command the admiration and respect of all.

Floreat Wood's Holl.

EVERY biologist who is still young enough to be enthusiastic, looks with eagerness about this time of year for the arrival of the volume of "Biological Lectures" from the Marine Biological Laboratory, Wood's Holl, Mass. The volume for 1898 (Boston: Ginn and Co., 1899, pp. 343) has just arrived, in good time for the Christmas holidays, when one can enjoy its stimuli with a less preoccupied mind. One cannot help feeling that the intellectual atmosphere of Wood's Holl must be bracing, the lectures are so vigorous.

The volume begins with a lecture by Professor E. B. Wilson on the structure of protoplasm, which we have already noticed. "The evidence indicates that alveolar, granular, fibrillar, and reticular structures are all of secondary origin and importance, and that the ultimate background of protoplasmic activity is the sensibly homogeneous matrix or continuous substance in which those structures appear." Wilson is also the author of the second lecture on cell-lineage and ancestral reminiscence—a strong plea for the acceptance of cell-homology. The third lecture on "adaptation in cleavage" is by Frank R. Lillie, who seeks to show that the special features of the cleavage in each species are as definitely adapted to the needs of the future larva as the latter is to the actual conditions of its environment. Professor E. G. Conklin discusses in the fourth lecture protoplasmic movement as a factor in differentiation, showing how delusive it is to consider the cell as if it were merely static, since movements of the cytoplasm play a very im-

portant part in developmental processes. In the fifth lecture Mr. A. L. Treadwell discusses equal and unequal cleavage in Annelids, in regard to which he seeks to show that equality of cleavage is not an indication of lack of differentiation in the ovum, for definite cells appear at definite places and at definite times, just as accurately as in unequal cleavage. The sixth lecture, by A. D. Mead, is more technical, dealing with the debatable question of the origin and homology of the prototroch. In the seventh lecture Miss Cornelia M. Clapp discusses the relation of the axis of the embryo to the first cleavage plane, and reaches "the only reasonable conclusion" that while the first cleavage plane may coincide with the median axis of the embryo, as Roux and others have shown, it is not a constant rule in any single case, much less a universal law. Dr. Thomas H. Montgomery, jun., recounts his observations on various nucleolar structures of the cell, and shows at least that both false and true nucleoli are structures of manifold complexity, in regard to which our knowledge is very vague. Dr. Watase follows with a lecture on protoplasmic contractility and phosphorescence, in which he gently leads up to the conclusion that the true physical basis of phosphorescence finds its closest analogue in the common phenomena of heat-production, and is as extensive as life itself. Professor T. H. Morgan discusses in the tenth lecture some problems of regeneration, showing that it is not easy to solve them all by quoting Lessona's law, or repeating the words "natural selection." In the eleventh lecture Professor Bumpus, who has previously made good use of sparrows, shows that they are subject to discriminate elimination. The twelfth lecture by Professor Jacques Loeb, on "The Heredity of the Marking in Fish Embryos," has been noticed separately.

The late Mr. W. W. Norman, whose loss to science is deplored, was the author of the thirteenth lecture, which shows that reactions of lower animals upon injury furnish no safe evidence of pain-sensations. Professor W. B. Scott discusses North American ruminant-like mammals in his accustomed style, and then follows a fine essay by Professor W. M. Wheeler on Wolff and the Theoria Generationis. But, in some ways, the most impressive lecture is the last, in which Professor Whitman discusses animal behaviour, and furnishes a notable contribution to comparative psychology.

The charm of these lectures may be partly due to the circumstances of their delivery, but it is doubtless mainly due to the fact that each is an expression of personal work and personal interest. One cannot but be grateful to the Laboratory at Wood's Holl, which has been the stimulus of the fine series to which this volume is added.—Floreat Wood's Holl.

Asterionella.

THE organism to which this elegant name pertains is a diatom recently investigated by Messrs. G. C. Whipple and D. D. Jackson (*Journal of the New England Water Works Association*, vol. xiv. No. 1). It causes trouble in water-supplies by producing objectionable tastes and odours. It is common in Massachusetts waters, and its recent occurrence in the Brooklyn supply led to the investigations here recorded.

The shape of this diatom resembles that of a humerus, and several cells unite to form star-like clusters. The only species is *A. formosa* (Hassall), but many varieties have been observed. The article itself must be consulted for details of structure.

The authors suppose that they saw spores or spore-like bodies in the cells, but they did not observe any of these spores (?) developing.

The diatom is widely scattered over Europe and North America, and is found in large ponds, lakes, and reservoirs, where comparatively clear water is stored.

It is said to be more abundant near the surface than in the depths. Normally it occurs in the spring and autumn, that is, regularly after periods of stagnation; but it is in ground waters stored in open reservoirs that it attains its greatest development. By ground water is meant water which has percolated through the ground.

The numbers of *Asterionella* vary from 1000 to 6000 per cubic centimetre of water. The odour at first is aromatic, then it resembles that of geraniums, and finally it becomes very fishy. The smell varies with the number of organisms in the water, and is due to a substance analogous to the essential oils.

A chemical analysis was made, and the mineral matter found to be 57 per cent of the dry weight of the organism, and of this nearly 50 per cent is silica, which is present to a greater extent in ground than in surface waters, hence the greater prevalence of *Asterionella* in the former.

The only practical suggestion possible is that reservoirs may be so designed as to be easily isolated and cleaned whenever necessary.

New Mice from St. Kilda.

MR. BARRETT HAMILTON has recently described [*Proc. Zool. Soc.* 1899] two new species of mice from St. Kilda, and his paper is of interest in its bearing on the *rôle* of isolation as a factor in evolution. At the same time, since mice are very common animals whose variation-statistics could be readily procured, one cannot at this time of day accept these two alleged new British species as securely based unless they are very thoroughly compared with the variations of *Mus sylvaticus* and *Mus musculus*. Let us illustrate our difficulties.

Alleged New Mice.

The length of the head and body of the largest St. Kilda specimen of *Mus hirtensis* n. sp. is 107 mm. for the male and 110 mm. for the female. This is exactly the size of a full-grown *Mus sylvaticus* in Elginshire. The skull of an Elginshire specimen just measured (apparently not an old one if we judge from the teeth) is 28 mm. long, 1 mm. less than the largest St. Kilda skull. The differences in ears and tail do not impress us, and still less those of colour. Even in one county *Mus sylvaticus* shows considerable diversity of coloration. At this season, when they sometimes come indoors to supplant the house-mouse for a time, specimens are trapped without a speck of yellow or brown on the side of neck or belly, while others are of a nearly uniform reddish colour on their upper parts with a very distinct line of demarcation between the white belly and sides. These are minor differences, giving no evidence of more than "individual variation." It may be, indeed, that they are merely "individual modifications" *sensu stricto*.

In Texan cornfields *Mus musculus* sometimes assumes in summer the reddish colour of some native species of Muridae, while the belly often becomes white or nearly white; the same species caught in Elginshire in October sometimes has the belly almost of the same colour as the back. It seems impossible to regard these as even varieties.

To illustrate further. A collection was made of an American species of *Cricetus* (*Hesperomys*), and the individuals were kept in captivity for a year or two. They varied in size, but did not vary much in colour, which was predominantly brownish grey. A fresh capture, however, was reddish, and suggested for the moment—we are all open to the temptation—a new variety. After some months of captivity it changed to the normal colour of the species. In all probability the original difference was simply the result of "modification."

It would be interesting to trap in Sutherland and Skye to see whether individuals of the *Mus hirtensis* type are not to be found there, for it is possible that the alleged new species is not the outcome of prolonged isolation, but was imported in hay or straw for the minister's horse a century ago.

The other form *Mus muralis* n. sp. is interesting on account of its colour, but as to its skull characters it appears to us that they will be found in perhaps every tenth *old* specimen of *Mus musculus* that comes to hand.

Our point, however, is apart from these details. It is that when we are dealing with forms for whose characters it would be easy to formulate variation-curves, this should not be neglected by those who would substantiate their claim to add new species to the British fauna.

British Mammals.

THE mammals in Britain are so few compared with other components of our fauna, that one naturally expects great accuracy in the descriptions which experts furnish. There may be better things than great accuracy, but it is at least a preliminary essential, and it is by no means always realised even in regard to British mammals. Which is disappointing.

Without ourselves claiming any infallibility we may illustrate our disappointment—made keener by our gratitude—by referring to a well-known handbook which seems to us to require a second edition. The author says that the common squirrel has a head and body about $8\frac{1}{3}$ inches in length, but every squirrel-catcher knows that a full-grown squirrel has a head and body about 10 inches in length. The picture given of the common (?) squirrel shows an animal with a tail longer than the head and body!

Of *Mus sylvaticus* the author says that it has a head and body about $4\frac{1}{6}$ in. long; the fraction suggests great accuracy, but a full-grown specimen in Elginshire often has a head and body $4\frac{1}{3}$ in. long. Of *Mus flavicollis* it is said "head and body $4\frac{1}{2}$ in. long," while of the field vole it is stated "length of head and body about $3\frac{3}{4}$ to $4\frac{3}{4}$ in. long," which surely suggests that field voles vary greatly in size, while field mice do not. Which is not the case. It is possible that the alleged species *Mus flavicollis* may be distinct from smaller varieties of the wood-mouse found in England, but in Scotland there are abundant intermediate forms, some of them as "large and handsome" as *Mus flavicollis*.

We may be making some mistake, but we are puzzled elsewhere, as when the author says "with the exception of the mouse-coloured bat, the Noctule is the largest of the British members of the order," and gives the length of its head and body as about 3 inches. But he states the length of the head and body of the mouse-coloured bat at $2\frac{1}{2}$ inches.

The author gives twenty-six pairs of teeth as the maximum in the common porpoise, but a male's skull in our possession has thirty pairs in the upper jaw. Of Sowerby's whale the author says "general colour white above and black beneath," but he must have seen the beast belly uppermost, for, when white is present, it is beneath, not above. The adults of both sexes which we have seen in the flesh had no white whatever, not even "white vermicular streaks." It is remarkable that one very distinct species of Cetacean is left out of the handbook altogether, though, judging from the number of skulls in collections, it is not the rarest one. It is needless to say that we make these remarks in no cavilling spirit, but merely to show that even in the works of experts the standard of accuracy is still not quite high enough.

Phylogeny of the Rodents.

THE two preceding notes may be said to have dealt in great part with little details about Rodents, and it is at once relevant and pleasant to direct attention to a recent work which deals with Rodents as a whole. We refer to Tycho Tullberg's great work, "Ueber das System der Nagethiere. Eine phylogenetische Studie." (K. Gesellschaft der Wissenschaften zu Upsala, 1899, pp. 514, 57 plates.) Beginning with an introduction which discusses the canons of phylogenetic inquiry and the general problem on hand, the author passes to a detailed statement of his anatomical results. On the foundation furnished by these he rears his phylogenetic system, proceeding in an orderly way which it is a pleasure to follow, discussing adaptation after adaptation, and the possible causes of various lines of structural change characteristic of the sub-orders and families. The fourth part of the big book deals with the distribution of Rodents in the past and present. He attaches little importance to the alleged affinities between Rodents and Marsupials; he emphasises the contrasts between Duplicidentata and Simplicidentata, but does not think that these are inconsistent with the view that both arose from a common pre-Rodent stock; and finally he suggests a genealogical tree of the order. To discuss his decisions on affinities in brief compass would be impossible, but the work is impressive as a phylogenetic study in which a vigorous attempt has been made not only to trace the possible steps in the evolution of an order, but to detect the possible causes which determined the direction of these steps.

Phylogeny of Rust.

THE origin of the rust fungi has recently given rise to a considerable amount of discussion, and Professor Dietel, in an interesting paper (*Bot. Centralbl.* lxxix. Nos. 3-4), considers the question of their descent from one or more plurivorous forms—forms, that is, which inhabited indifferently hosts belonging to the most widely different families of flowering plants. At the present day, however, only one species, a *Cronartium*, is known to retain this peculiarity, having been shown by Fischer to be capable of life on plants belonging to both Ranunculaceae and Asclepiadeae. But Professor Dietel adduces a mass of collateral evidence which seems to show that the balance of probability at least lies on the side of his hypothesis. It would indeed be difficult to account on any other grounds for the close morphological resemblances existing between forms which, while biologically distinct and inhabiting plants belonging to the most widely different families, are at the same time almost indistinguishable by any

other features. *Triphragium clavellosum*, for example, is confined to *Aralia nudicaulis*, and differs from *T. Credelae*, which lives on *Credela chinensis*, merely by the size of the spores, a difference which does not exceed the dimensions of a single micron.

Such forms must obviously be looked upon as having sprung from a common ancestor, which in this case must have lived on both hosts indifferently, especially as the two species agree in the possession of characters which distinguish them sharply from all other Triphragia.

Another example is supplied by Leptopuccinias like *P. Arechavaletae* living on Sapindaceae, *P. heterospora* on Malvaceae, *P. Elytrariae* on Acanthaceae, and *P. Lantoneae* on Verbenaceae, all of which closely resemble each other in the form of their spores and spore-beds; while all possess in common such distinctive characters as the preponderance of unicellular teleutospores, isolated individuals of which may reach a much greater size than their fellows, and the occasional occurrence of isolated bicellular spores which also vary in size, and the septum of which is often oblique, while the only morphological differences are to be found in slight diversities in the size of the spores and in the thickness of their walls.

Further evidence of the same kind is furnished by the only three Puccinosiras known, and may probably be found in a number of other heteroecious forms.

A striking morphological resemblance is also observable between certain Leptopuccinias and the teleutospores of heteroecious species parasitic on widely different plants, but possessing aecidia which live on the same hosts as the Leptopuccinias in question, *e.g. Puccinia accidii leucanthemi*, which forms aecidia on *Chrysanthemum leucanthemum*, gives rise on *Carex montana* to teleutospores which closely resemble those borne by the Lepto-form *Puccinia leucanthemi* on the former host.

Professor Dietel cites a large number of such correspondences, and believes that they point to the origin of the heteroecious and Lepto-forms in a common ancestor inhabiting such widely different hosts as Carices and Composites, while, on the other hand, Professor Magnus is of opinion that the resemblance is purely accidental, and ascribable to the great similarity existing among Leptopuccinias as a whole, owing to adaptation to their peculiar mode of life.

The coronate Puccinias, including, along with those heteroecious species which form their aecidia on *Rhamnus*, the two Leptopuccinias also living on the same host, and *P. Festucae*, which forms its aecidia on *Lonicera*, are distinguished from all other Uredines by the possession on the teleutospores of a crown of processes which appear to be devoid of adaptational significance, and must be considered as pointing to a common ancestry for these forms, especially as the only other Uredine inhabiting *Lonicera* is *Puccinia longirostris*, in which the crown is replaced by a single long process on the apex of the teleutospore, but

which resembles in all other particulars one of the Lepto-forms inhabiting *Rhamnus*. Fischer prefers the view that in this case the ancestral form was capable of completing the whole cycle of its life-history, as well on grasses as on various species of *Rhamnus*, and that its descendants became specialised so as to form either aecidia on *Rhamnus* and the uredo-teleutospore generation on grasses, or the aecidia was dropped and the uredo-teleutospore generation alone persisted on *Rhamnus* as in the Leptopuccinias in question.

As, however, these give rise to several generations on the same host in the course of each year, Dietel is unable to recognise any sufficient cause for the disappearance of the aecidial generation, and believes a more probable view to be that the ancestral form only bore teleutospores, and that the uredo and aecidial generations originated at a later phylogenetic stage, a hypothesis which receives some support from Brefeld's well-known views regarding the origin of the Uredines from the Auricularias, a saprophytic group which possesses no spore form comparable with either aecidio- or uredo-spores, both of which may have originated as an adaptation to a parasitic mode of existence, though not necessarily on all the host plants inhabited by the parent form.

Ferments in Fungi.

THE fat-splitting ferment first obtained in a pure state by Professor Green during his classical researches on the germination of castor oil seeds, or at least a ferment possessing similar properties, has just been obtained by Mr. R. H. Biffin (*Annals of Botany*, 1899, p. 363), from a fungus which he was fortunate enough to find growing on the endosperm of a germinating cocoa-nut, and which apparently belongs to the Hypocreaceae, though to which section of the family it must ultimately be referred remains undecided, owing to the constant sterility of the perithecia, in which no ascospores have as yet been found, though chlamydospores and sickle-like microconidia are abundant on the mycelium. The fungus grows freely on sterilised slices of cocoa-nut and Brazil-nut endosperm, as well as in cocoa-nut milk and similar media, with the result that the oil which these contain gradually disappears, being decomposed into glycerine and fatty acids, the former of which is absorbed by the plant and forms its source of carbohydrate food material, while the latter accumulates in the fluid and increases its acidity.

Mr. Biffin has succeeded in isolating the ferment by the usual process of extraction with water and precipitation by means of alcohol, when a white substance was obtained, which, when re-dissolved in water, furnished a solution possessing the properties of the fungus, in so far as these are concerned in the decomposition of fats.

The same fungus appears also to secrete a cellulose-dissolving ferment, as its hyphae may be seen to penetrate with ease the walls of the endosperm cells.

More Sports.

THE *Annals of Botany* (Sept. 1899) contains an interesting paper by Professor H. de Vries on the inheritance of sports. He has obtained a race of *Dipsacus sylvestris* in which the leaves are all spirally arranged, instead of being in the opposite-decussate system, typical of this plant in general. The original parents were two individuals raised from seed sown in 1884; these were carefully isolated, and from their seed 1650 plants were obtained in 1886, *Dipsacus sylvestris* being a biennial plant; but of this large number only two retained the spiral phyllotaxis. These were allowed to seed while all the others were destroyed before flowering, and the third generation, composed of about the same number of plants, contained sixty-seven twisted individuals, or about four per cent. The fourth generation gave ten per cent, but, owing to an accident, its seed could not be employed, so that another fourth generation was raised from the remaining seed of the third generation sown in 1891, and resulted in a yield of thirty-four per cent of twisted individuals, a percentage which has not been greatly exceeded in subsequent cultivations.

The gradual rise in the percentage of good plants is accounted for by improvements in the cultural methods, especially with regard to the amount of space put at the disposal of individuals, while the richness of the soil and the time of sowing are likewise factors of essential importance in the production of successful results; in short, the perpetuation of such useless if not harmful variations requires the presence of an environment as favourable as possible to the life of the plant.

A Pontifical Plant.

IT cannot be laid to the charge of *Natural Science* that it has been prone to get excited over the creation of a new species, but our esteemed contemporary *Science* (October 20, 1899) has called our attention to one which affords us a purr of delight. The reference is to the *Daily Chronicle*, where botanists might naturally overlook it. As to the description of the new species, it is given in somewhat unconventional language, but this may be pardoned in the new departure of a London Daily. "The Pope takes great interest in an electric plant, to which he has given the name 'Officina Electrica

Vaticana Alessandro Volta' in honour of Volta. A few days ago His Holiness made an inspection of these plants, and the employées of the Vatican Gardens were presented to him by the Chief."

Neptuneopsis.

THE opinion is not infrequently expressed that it is hopeless now to expect novelties among the larger mollusca, and certainly the great majority of new species recently described have been of small dimensions. Now, however, as if to show how far from exhausted are the riches of the sea, we have a handsome gastropod with a shell over 16 cm. in length from comparatively shallow water (33 fathoms) off the Cape of Good Hope. It constitutes a new genus, to which the name *Neptuneopsis* has been given by Mr. G. B. Sowerby, and it has been placed in the family Volutidae, though it seems to have relationships also with the Buccinidae, Fusidae, and Cancellariidae. Perhaps, however, the greatest mystery regarding this new shell is that its publication (with a handsome coloured plate) has been undertaken by the Department of Agriculture of Cape Colony.

A Note on Inheritance.

"UNTIL recently," says Professor Jacques Loeb, "heredity has been treated chiefly as a problem for whose solution one single theory or one single principle was considered possible and sufficient." Various theories have been propounded, but none have been generally accepted. "They overlook the fact that heredity is a collective term for a series of heterogeneous circumstances which cannot possibly be explained by one principle." A more analytical study "has led to the conception that very different circumstances determine the various details in heredity," and the author gives the results of one of his studies prompted by this conception (*Biol. Lectures Wood's Holl* for 1898, pp. 227-234, 6 figs.).

But before we report on this, may we suggest that it would be clearer to agree that heredity is the most convenient term for *the relation between successive generations*, for then it is self-evident that there are several quite distinct problems to be faced. There is the question of the material basis of inheritance, whether in germ-cell or bud or otherwise; there is the question as to how this material basis has come to be what it is—capable of reproducing an organism more or less like the parent; there is the detailed comparison of one generation with another, and the attempt to distinguish how far the resemblances

and differences are due to real transmission of heritable qualities, and how far to similarity in the induced "modifications"; there is the analysis of the inheritance by statistical and experimental methods, the biggest result of which has been Galton's law; and there is the at present almost unassailable problem of conceiving how the heritable qualities work their way into realisation during the process of development — a problem that leads us away from the strict problem of heredity to that of "the principles of development." In short, we hardly think that the serious student of heredity has ever thought that he was facing one problem to which it might be expected some day to find one answer. In any case, we cannot admire the ingenious observer's phraseology when he says that "what we call heredity is composed of very heterogeneous constituents." He speaks of the so-called theories of Eimer as "nothing but a play on words," but might not Loeb strengthen his case by taking his own words more seriously?

The particular problem which Professor Loeb discusses is that of the tiger-like markings in the yolk sac of the embryo of the fish called *Fundulus*—a subject in regard to which he has previously published results. The origin of the coloration is as follows:—black and red chromatophores are found on the surface of the yolk sac; they gradually creep upon the blood-vessels and ensheath these, exhibiting chemotropism due especially to the oxygen of the blood, or stereotropism (another brave word), or both. "The heredity of the markings is, therefore, in this case determined by a stimulus which the blood-vessels exert upon another tissue, namely, the chromatophores. Both tissues are formed rather independently of each other, but from the fact that the chromatophores must creep upon the blood-vessels, and that the latter have a hereditary arrangement, the marking becomes hereditary too. This contradicts those theories of heredity which try to derive all the peculiarities of the animal from corresponding peculiarities of the sexual cell, for instance, Weismann's theory." But this is lamentable confusion; for no one surely has supposed that there are not analysable immediate conditions operative at every stage of development: the point of Weismann's theory is that the inherited organisation determines the particular occurrence and sequence of these conditions, and is thus the primary though not the immediate cause of the results.

Loeb gives an interesting figure of the tail of an embryo, in which the chromatophores are seen to have crept upon the median artery while the vein remains free. This suggests that the oxygen of the blood may be one of the causes that force the chromatophores to creep upon the blood-vessels. But this is not the whole reason, for wherever a vein is isolated they creep upon it too. Moreover, the back of the embryo is coloured black by pigment cells which follow the brain and the spinal cord.

The observations are interesting, but they appear to us to have to

do with the conditions of development, and not with the strict problem of heredity. Nor is the necessity for such investigations by any means a new discovery, for many years have passed since Professor His protested that "to think that heredity will build up organic beings without mechanical means is a piece of unscientific mysticism."

The Cell as a Unit of Organisation.

THE view has often been expressed that the functions of a cell depend upon the mutual relations of its component parts. That is to say, there is a "cell-firm," in which the most important partners are the nucleoplasm, the cytoplasm, and the centrosomes, a firm which owes its power and its success to the mutualism of its partners. Dr. F. Schenck has recently published an interesting paper discussing this conception ("Physiologische Charakteristik der Zelle," pp. vi. + 123. Würzburg: A. Stuber (C. Kabitzch), 1899. Price 3 marks), in which he comes to the following conclusions:—

Not every cell can be called a physiological individual, such as a Protozoon is, for there are cells which are merely parts of a physiological individual. The process of vital combustion, and what directly depends on this, cannot be regarded as dependent on the co-operation of the characteristic components of the "cell-firm," and to a certain degree even assimilation is independent of the particular organisation. The latter is, however, implied in growth, regeneration, and differentiation; in these processes the components of the cell combine to form a unit of organisation. But sometimes the result cannot be explained from within the cell itself, but depends upon the physiological relations between the cell and the larger system of which it forms a part. The cell-structure of an organism is the structural expression of a functional division of labour in which the nucleus plays the more important (organising) rôle, while the cytoplasm is its medium reacting to external stimuli. Processes of division, in which the third important partner—the centrosome—has an influential rôle, have for their end the distribution of nucleoplasm and cytoplasm in such proportions that appropriate cellular functions continue. There is nothing novel or startling in these conclusions, but they are temperately expressed and illustrated in considerable detail; and we can heartily commend the publication to those particularly interested in cell-problems.

The Biological Corner of a Natural History Museum.

PROFESSOR L. CUÉNOT discusses in *La Feuille des Jeunes Naturalistes* (xxix. 1899, pp. 195-197) the possibility and utility of collections to illustrate facts and problems of general biology. He instances the cases in the entrance hall in the British Museum (Natural History), and some illustrations which he saw in the University Museum in Cambridge, but he protests, like Herrera, that what has hitherto been the exception should in the future prove the rule.

He takes the chapters in *L'Année Biologique*, and suggests that, although one must not expect too much in museum illustration of these, one may reasonably look for more than is at present offered. At Nancy he has himself tried to realise some of his ideals. Regeneration, parasitic castration and peculiarities of sex-inhibition, homochromy and other protective adaptations, variation, sexual dimorphism, convergence, and the like may be vividly illustrated without great difficulty. Even heredity he would illustrate by generations of mice, and the recapitulation-doctrine by placing young Comatulas in their stalked stage beside *Pentacrinus*. There is obviously no difficulty except that of time and money, which applies to other kinds of exhibits, and the pains of thought which inhibit many of these valuable suggestions. It is only fair to note, however, that the number of these biological exhibits is rapidly increasing both at home and abroad.

Linné's Type Specimens of Fishes.

ONE of the many excellent outcomes of Dr. Günther's presidency of the Linnean Society will be seen in his Anniversary Address for May last, just issued. In this address he deals with the fish preserved in Linné's own collection, which has been in the possession of the Society for about a century. How little they have been valued by the Society may best be gathered from the fact that Dr. Günther records, "in order to render them more secure in the future, your Council has ordered them to be transferred [from loose sheets of paper] to dust-proof glass-topped boxes." One only hopes that Dr. Günther will see that every precious Linnean specimen is placed in a glass-topped box before he leaves the presidential chair; it is difficult to understand why this was not done years ago.

The fishes owned by Linné consist of 168 skins, and came from three sources, Scandinavia, Germany (chiefly freshwater), and South Carolina. They are all preserved like plants in a herbarium, and on the sheets of paper are usually notes in Linné's handwriting, while those from South Carolina usually have a band of paper round the tail,

inscribed by Dr. Alexander Garden, who sent the specimens to Linné. Of these 168 skins about 40 are "types," and nearly all these came from Dr. Garden, and all of them are American.

Dr. Günther has given a careful description of each skin, the marks or writing upon it or upon the label or sheet of paper, and has added comments of his own on previous identifications, and other points of interest. Altogether a very excellent and valuable Presidential Address.

The Molluscan "Liver" So-Called.

To the student of the comparative physiology of the Invertebrates the word "liver" is a red rag. It has been applied to many different kinds of organs, and with its vertebrate connotation it has fitted none of them well. For a time, indeed, it seemed as if the recognised way of dealing with a puzzling organ was to "call it a liver and have done with it." But we have at least got beyond the stage of hypocrisy, if not of ignorance, and we speak of "the so-called liver." So at least do Messrs. Biedermann and Moritz in a recent study of the organ in question in Molluscs (*Pflüger's Archiv f. Physiologie*, lxxv. 1899, pp. 1-86), and it seems for the time a convenient device,—for the attempts to introduce such terms as "hepatopancreas," "poly-enzymatic gland," "mid-gut gland," "gastric gland," and the like have not been very successful. Do what we will, the "liver" is always with us, or with our students at least, and therefore it seems better to give it a slow death in the shackles of "so-called." But let us attend to the last news in regard to the function of this organ in the snail.

The so-called "liver" of snails contains three kinds of cells,—(*a*) secretory cells, whose secretion digests starch and cellulose in the stomach, (*b*) absorptive cells, and (*c*) lime cells. The two last accumulate stores of glycogen, fat, and perhaps some albuminoid substance. The lime-cells have especially to do with the storage of fat and calcium phosphate. The fresh secretion has no appreciable digestive effect on albuminoids. There is no absorption in the intestine, which is lined by ciliated and glandular epithelium; its fluid contents pass into the recesses of the so-called "liver" and back again. This appears to be the gist of the research, and it means another step out of obscurity.

Phylogenetic Senescence.

THOSE who know Professor R. Wiedersheim and his works will agree with us when we say that he cannot be blamed, as human anatomists often are, for undue preoccupation with the static aspects of man's body. In his essay on retrogression and in his book on the evidence

of the past in man's present structure, he impressed us with the idea that we carry about with us a museum of relics, that some of our structures are at present in a transition-stage of function-change, and that some parts are even progressing.

In a recent essay, entitled "Senescenza filogenetica" (*Rivista di Scienze Biol.* 1899, Fasc. iv. pp. 1-7), he has pointed out (1) that organs in process of phylogenetic regression, *e.g.* the tips of the lungs, the caudal end of the spinal cord, Morgagni's pouches in the larynx, and the posterior molars, have their weak spots, their *loci minoris resistentiae*, where they are peculiarly disposed to disease; (2) that organs and parts of organs in process of function-change, *e.g.* the thyroid, the thymus, the inferior nasal muscles, and perhaps the tonsils, are likewise peculiarly open to attack; and (3) that progressive parts, such as certain muscles and bones, are strong in their progressiveness, and less liable to disease than the parts in the two preceding categories. One cannot help wondering with another reviewer, Dr. W. A. Nagel, whether the last statement will hold good in regard to our brains, which we fondly hope are also on the line of progress.

There appear to be two distinct ways of interpreting this "phylogenetic senescence." On the one hand, we have to consider the immediate physiological conditions, *e.g.* of diminished blood-supply and weakened innervation, which may lessen the resisting power of a dwindling organ. On the other hand, we have, with Weismann, to go further back, and consider the possibility of a germinal struggle and selection among the stronger and weaker determinants, and supplementary to both interpretations there is the normal action of natural selection.

Studies in Plant Morphology.

SCHUMANN of Berlin has recently published through Engelmann (Leipzig) a second part (pp. 207-313) of his "Morphologische Studien." The studies are of a special and somewhat abstruse character, dealing with flower- and leaf-arrangement and including questions of development, mechanical conditions and the like. They will be read with interest by the somewhat limited number of botanists who can appreciate or follow such discussions. The first (No. III.) deals with the vexed question of the peculiar inflorescence in the Boraginaceae and Solanaceae, and is a criticism of a publication by Kolkwitz. The second (No. IV.) is an account of the branch- and floral-development in a commonly grown greenhouse plant, *Scirpus setaceus* (*Isolepis setacea*). No. V. deals with the leaf-arrangement in screw-pines, while No. VI., occupying two-thirds of the whole part, and entitled "The Shifting of Organs on Growing Shoots," is mainly a criticism of Schwendener's views on the same subject.

Trees in Winter.

By P. Q. Keegan, LL.D.

The external aspect of our forested and scattered trees in winter is very familiar, but the mysteries of their interior being at that season are wrapped in obscurity, and demand for their elucidation all the analytical acumen and manipulative skill that can be bestowed upon the subject. Up till within the last few years neither out-door naturalists nor arm-chair faddists cared very much about the secret arcanum, the slumbrous hibernating activities, or rather passivities, of the denizens of the forest while enduring the sharp rigours of the deepest winter. They seemed only to sleep, a few appeared to be absolutely dead, their sprouting germinative activity was no more, and save for the mystical entanglement of the leafless boughs and the picturesque intricacy of the bud-studded twigs, there was no basis, no attractive feature anywhere apparent to call forth physiological or artistic interest. If the life of the forest was to be studied and adequately comprehended, it must be done, as was thought, when buds had burst and leaves had shot forth and flowers had blown into full expansion, when life was everywhere quivering and tingling in the running sap and swollen root and stirring leaf. Such was the impression; but it was narrow and one-sided, it ignored the best half of the affair, it disdained the law that organised matter adapts itself to circumstances, to the wintry chill as well as to the sultry glare, that it operates by counterparts, so to speak, neither of which is complete, but each a supplementary constituent of the grand totality.

The justification for the foregoing remarks will, I think, be found by any one who cares to make himself conversant with the history of scientific research anent the winter life of our trees. Previous to the year 1870 a few plant analysts and botanical chemists had investigated various parts and organs such as barks, buds, etc., gathered during the winter season; but at all events, in 1871 Richard observed that in the month of February there was a deficiency of starch in certain twigs of willow, linden, and birch, which cases, however, he considered to be mere exceptions to the law broached by Mohl, founded by Hartig and Sachs, and generally held true at the time, viz. that the reserve starch

undergoes no change in winter. N. J. C. Muller was the first to observe the disappearance of the rind starch in winter, and he thought that it migrated into the wood. Russow, in the winter of 1880-81, examined the barks of ninety-two different tree stems up to sixty years old, and found starch only in ten kinds, but as compared with the autumnal content it showed a great diminution; experimenting again in the colder winter of 1882, he found that in all species of tree the starch had disappeared up to isolated traces, it being transformed principally into fat-oil; on the other hand, he came to the conclusion that the starch had all the time remained unchanged in the wood of all the species investigated. He found, moreover, that towards the end of March the rind starch had been copiously formed again, *i.e.* long before the bursting of the buds, the suppression of the carbohydrate thus lasting from November till April. In 1884 Baranetzky and Grebnitzky published the results of their researches. They found that not only the rind starch but also the wood starch was reduced in winter, and may even disappear altogether, *e.g.* in lime tree, fat-oil stepping into its place; on the other hand, in hard-wooded trees, while the starch vanishes entirely from the rind, it remains, though somewhat reduced in quantity, in the wood. In 1890 Dr. A. Fischer confirmed the views of these observers, and further investigated the method and course pursued in the process of the starch dissolution in fat-trees during the autumn. He emphasised the opinion that the entire wood-starch of the younger twigs in fat-trees is transformed on the spot, *i.e.* the principal mass of the starch undergoes no translocation. He was also disposed to conclude that the greater part of the fat in the older wood of certain trees is never changed at all, whereas that contained in the rind disappears almost entirely in spring and summer. He further recognised eight phases of starch transformation, viz. a maximum in October and in April, a minimum in December, January, and February, and again in the latter half of May, a dissolution in November and beginning of May, a regeneration in March, and a storing up from June till October. In 1891 Monsieur Emile Mer, who had studied the distribution of starch in the principal trees and indigenous shrubs of France, found that in the middle of November a great change had already been wrought, the starch had nearly all disappeared from the cortex and liber at least in the branches as well as in the middle and upper parts of the trunk; in the wood it had notably diminished in white-wooded trees, though still abundant in hard-wooded trees, while plants with persistent leaves hardly held it any longer save at the base of the stem and in the current year's twigs chiefly on a level with the buds. He found that the starch gradually passes from the wood into the liber, first from the medullary rays, then from the wood parenchyma, and finally from the medullary sheath and pith; the rays of the young liber are the last to yield up the vanishing starch. Apparently this absorption must needs, he

thinks, be attributed to the respiratory combustion exerted by the woody and liberian tissues from the moment when the leaves have lost their assimilatory activity up till the beginning of the winter rest. "As long," he says, "as a certain degree of moisture remains in the tissues, life is maintained there, and it may occur that the starch reserve is entirely absorbed. In the same way, after the fall of the leaf, woody plants still continue for a certain time to vegetate and to respire; it is in the liber that this function seems to be most active and most persistent. It is not only a more or less complete absorption of the starch reserve that is produced in autumn, it even works a profound change in the distribution—due to this that the foci of attraction are displaced. It is known, in fact, that starch is borne always to the points where vitality is most developed. Now in this season the only regions where there still remains a residue of vegetative activity are, on the one hand, the buds which the young branches bear, on the other the roots whose vegetation is prolonged for a certain time after that of the aerial organs exposed to the first cold. In proportion as the season advances the respiratory combustion slackens, and from the moment when the vegetable enters into the period of latent life, the distribution of the starch remains stationary during nearly three months" (*Comptes Rendus*, vol. cxii. for 1891, p. 964).

I think it would be difficult to quote or translate a passage which reflects more faithfully and lucidly the inevitable results of true scientific observation and experiment upon the veritable scientific intelligence. The facts are interpreted aright and referred back, so to speak, to their real, *i.e.* their physiological cause. It has been maintained that the winter period of rest of our deciduous trees is not dependent on external conditions, such as temperature or moisture, but on internal changes, and especially on such as enable the starch-containing cells to transform their starch into sugar. Sachs thought that possibly there must be a very slow production of ferments before the buds can develop in spring, as they cannot by any means be caused to develop in autumn or beginning of winter, although meanwhile their reserve materials are not chemically changed. Nevertheless, a study of the actual condition of affairs within the veil of mystery that enwraps the winter forest, reveals the groundlessness of the opinion that the reserve materials are chemically unchanged. In point of fact, we discern that the protoplasm contained in the delicate, colourless tissues of the bark becomes very rich in fatty matter, probably operative as a resistant to the extremes of cold. The starch, moreover, which in most of our trees is laid up in the wood and in special reservoirs below the buds, is during the hard season very rich in substance and poor in water, the grains seem to be smaller than in summer, and amylodextrine seems to accompany it, *i.e.* altogether it is hardly in a condition well fitted for chemical transformation. No doubt glucose or other combustible carbohydrate may gradually all the while be

entering into solution in the cell sap; but the quantity thereof must be extremely small, perhaps not much more than sufficient to forefend the utter and final extinction of the feeble spark of life that continues to glimmer amid the bitter cold and benumbing surroundings. Moreover, although it is recognised that ferments are the products of cells in process of disorganisation, there is some doubt whether even this process goes on in the dead waste and middle of winter. Rather a universal torpor seems to reign in the domain of plant life, and, in a general way, "as you were" is the word of command from November till March. On these grounds, therefore, and for other reasons too abstruse to be succinctly recited, I am disposed to conclude that the winter period of rest, even in our evergreens, does actually and principally (I do not say entirely) depend on the external conditions to which the plant is subjected. In the case of the Coniferae, their limitation of growth towards the north is due to dry winds on sunny days in winter stimulating transpiration at a time when the roots can draw no fresh supplies of moisture from the frost-bound soil. Hence in the evergreen leaves of this order, special protective contrivances against excessive transpiration are indispensable. In our deciduous dicotyledonous growths, on the other hand, these special defences are apparently incompatible with that full and free activity of the chlorophyllian protoplasm in summer which is necessary to build up characteristically hard woods.

Descending now to particulars, it is proper to mention that what Fischer has termed fat-trees are those which are soft-wooded, and contain at the period of the starch minimum in winter (December, January, and February) no starch at all in the rind, wood, or pith, *e.g.* Scotch fir, birch, alder, poplars, lime, *Robinia*; in spruce fir, larch, yew, juniper, etc., the wood never becomes completely devoid of starch, but even in these fat predominates in the wood in winter. Starch-trees, on the other hand, are hard-wooded, and while in winter the starch disappears completely from their bark and pith, it remains almost unchanged in quantity in the wood and medullary sheath. The ultimate cause of these differences seems to be that the assimilatory activity of the foliar organs of the trees in the first category is not so active as it is in those of the other. More starch is produced in the leaves of the latter, the starchy reserves of the medullary rays and wood parenchyma are more redundant and not so readily exhausted; hence vitality is more developed, the annual rings are broader, and the excess of plastic substance is used up in the thickening of the autumn zone of wood, the whole contributing to raise its density and hardness considerably as compared with that of firs, pines, and other fat-trees. My own investigations lead me to consider that the wood of conifers is very poor in starch at all times, even in isolated trees developed in the highest noon of summer; while again, although at this season the wood of birch, alder, lime, etc., is very rich in starch, it, even before

the leaf falls, easily degrades and suffers what may be called a dextrine change.

Is the process of deassimilation likewise checked and brought to rest within the inner arcanum of our trees in winter? Do tannoids, resins, volatile oils, waxes, tannins, and coloured pigments continue to be produced as the outcome of the spent and exhausted energy of the chlorophyllian protoplasm? "Assimilation," says Mesnard, "may be very feeble and even be annulled completely, but deassimilation should not be null as it is indispensable to the proper functioning of the cell." Wigand, in a general way, declared that the young shoot in the condition of winter-bud contains no tannin, but has starch; he imagined that the tannin is changed into starch, and in that condition held as it were its winter sleep. In 1875 Oser concluded that the tannin of the current year's twigs of oak decreased in winter, it being possibly used up in a kind of internal respiration, the tannic acids being very easily oxidisable. In 1888 E. Schulze discussed the question, Are the leaves of evergreen trees emptied in autumn like caducous leaves, or are they filled with reserve materials like the other persistent organs? He performed numerous microchemical experiments, and concluded that only in Gymnosperms and in most Dicotyledons do the leaves serve as magazines of reserves during the resting period. He found that not only starch but fatty oil and tannin may still be detected in the winter foliage; sometimes tannin is found there alone, sometimes it exists along with starch or oil, but they are rarely found side by side in the same cell; moreover, when oil accompanies tannin, the cells which contain the oil are generally deprived of starch. Starch and tannin occur in the winter leaves of oak, holly, mistletoe, spindle-tree, etc., whereas those of ivy, guelder rose, firs, pines, etc., contain tannin only. G. Kraus carefully examined the youngest shoots of several trees and shrubs monthly during the winter, and found that the tannin of the twigs formed in the preceding vegetation period undergoes no change in the winter months, and hence Oser's idea of its mission as a respiratory material falls to the ground, and Schulze's and Haberland's opinion that it is a reserve substance is consequently unsatisfactory.

Nevertheless, I think there is some satisfactory evidence to prove that if tannin, *i.e.* the capital product of deassimilation, does not increase during the dead months of December, January, and February, it at all events develops to some extent; this is to say, by further exposure to the oxidising agencies of light and air it suffers dehydration, or a molecular rearrangement of its constituent atoms. For instance, it is during the wintry gloom that the leaves of ivy assume their brightest red, the buds of the Norway maple are red in autumn but become of a still darker red in the course of the winter, the holly berry never shows so ruddy a radiance as about the merry Christmas time, and many other illustrations may readily be recalled. Indeed, from the

analogy of the autumn manifestation of colorific effect investing the woodlands at a time when the assimilatory activity becomes dull and deadened, there is nothing unreasonable in the assumption that oxidising effects continue to be produced later on when only a feeble minimum of protoplasmic respiration remains as the last remnant of vitality. Even in dead leaves the glucose and other autoxidisable substances disappear, at least in part, as the result of the direct action of atmospheric oxygen. So that whether the process be regarded as either physiological or chemical or both combined, it would be absurd to imagine that a substance absorbing oxygen so readily as tannin does, can remain totally unaffected through fresh winds, sunny skies at times, and a small absolute content of aerial moisture. Judging from the analogy of the fruit, wherein tannin remains long and in some cases even is completely destroyed by oxidation, it would seem that the tannin of the winter boughs and leaves gradually becomes, as the season advances, more complex in composition, less easily crystallisable, and less soluble; possibly it takes up new carbon radicals, whereby, while retaining an analogous-chemical constitution, its reducing properties are not diminished.

It might be imagined that a property like wax-formation, suberification, etc., to which plants owe their great power of resistance to the effects of climate, would, if not specially prominent, be at all events not altogether suspended during the winter months. It appears, however, that even in these respects the palsying, life-consuming influences of cold are not arrested. "The resin and wax metamorphosis are probably conditioned by a slackening of the cellular activity," says Wigand. On the other hand, according to Uloth, who had carefully studied the wax-formation in *Acer striatum* and other trees, this process is not a physiological but rather a purely chemical one, requiring a peculiar condition of the cellulose, with the co-operation of light and of a certain high temperature, and hence takes place only during summer. "During the winter," he states, "as is seen distinctly after the fall of the leaves, the wax-forming process stands still in order to begin anew with the second spring entirely in the same way as before." This attestation is of some importance, inasmuch as it throws some light on the vexed question of the precise physiological character and position of a substance, the origin of which has proved rather a bugbear to all serious students of arboreal chemistry. My own impression is that wax, suberin, etc., represent the products of chemical decomposition (deassimilation) resulting from the specially vigorous and rapid activity of certain locally restricted non-sexual propagative cells, such, for instance, as compose the phellogen and the epidermis of young leaves; and this being so, the fact that this unwonted energy is arrested in winter becomes easily explicable, and is by no means extraordinary.

Not the least remarkable of the phenomena connected with the

winter rest of our trees is the lavish accumulation of oxalate of calcium in the buds and even in the pith of the young shoots. A section made even in October through the bud of sycamore, alder, or ash reveals an extraordinary state of affairs. What does it all mean? The cells seem reeking, as it were, with large or small crystals well and truly formed. In sycamore buds these are very large, while in those of the ash they are of all sizes apparently. A transverse section of the bud-scales of the latter tree shows a peculiar collenchymatous tissue filled with a thickly granulated plasma which invariably encloses among several tiny rodlets of oxalate of calcium, a large octahedron of the same substance. "The oxalate formed in the autumn in the buds is still unchanged in spring," says Wehmer. Kraus concludes that it is a reserve food material and not an excretion, and is taken up in spring; but after careful study and consideration I am disposed to conclude that it merely represents an oxidation product of the carbohydrates; in fact, it is the result of a specially active metabolism connected with the molecular rearrangement of certain carbohydrates while being subjected to an unwonted and extraordinary intensity of respiration.

PATTERDALE, WESTMORLAND.

Lacepède's "Tableaux . . . des Mammifères et des Oiseaux," 1799.

By C. Davies Sherborn.

In *Natural Science* for December 1897 (p. 432) there will be found a letter by me upon Lacepède's "Tableaux." These "Tableaux" are classifications of mammals and birds, and their interest consists in the fact that in them many generic names are used for the first time in literature. In the communication referred to above, I mentioned that an edition of Buffon had been discovered in which these "Tableaux" occurred, and that the volume containing these "Tableaux" was dated 1799. As this was the date of the original publication, and as the original publication had been lost sight of practically since it first saw the light, the discovery was apparently of considerable importance.

In June 1899 I received a kindly communication from Mr. C. W. Richmond of the United States National Museum, pointing out that according to the *Journal Typographique* of Paris, the edition of Buffon in question, although dated 1799, apparently came out in livraisons of two volumes a month, and that the particular volume containing the "Tableaux" (vol. xiv. of the Quadrupeds) was not published until October 1802. Mr. Richmond has since published the whole story in *The Auk*, vol. xvi. No. 4, October 1899, pp. 325-329.

Now let me deal with the Buffon first. The edition in question was published in 76 vols. 18mo, Paris, 1799-1809. It was printed by Plassan, and published by Saugrain. At the conclusion of the 35th livraison (vol. xiv. "Quadrupèdes," and vol. x. "Poissons"), that is in Oct. 1802, the entire work was purchased by the brothers Didot (*Journ. Typographique*, August 25, 1803, p. 358) and reissued with new title-pages, bearing their imprint instead of that of Saugrain, but carrying the same date, viz. 1799, the date in the original issue being expressed, "L'an vii de la République," and in the Didot issue as "An vii—1799." The Saugrain title-pages were torn out and the Didot title-pages pasted upon the guard thus left. Didot then issued in 1804 vols. xi.-xiv. of Poissons, and in 1809 vols. i. ii. of Cétacés, thus completing the 76 vols. The copy in the British Museum (Nat. Hist.) is of the second issue, with the exception of vol. i. of the "Quad.

Ovipares," which still bears the Saugrain title-page; Professor Newton has the greater part of an original set containing some of the Saugrain title-pages, which formerly belonged to Mr. de Winton, while Mr. de Winton has recently acquired a second set, bearing the Didot replacements. Now it seemed quite possible that the Saugrain issue was published as a whole at first, and proving unsaleable was afterwards issued in livraisons of 2 vols. per month; but it was not so. Mr. Richmond's authority (*Journ. Typ.*) is quite accurate, for it is proved to be so by a reference to the *Journal Général de la Littérature de la France*, which not only gives from month to month, practically the same information as does the *Journ. Typ.*, but definitely states on 7 Nivôse vii. (December 28, 1798) that Plassan would publish an 18mo ed. of Buffon by Lacepède, 2 vols. on the 1st of each month, beginning Floréal vii. (Ap. 20, 1799). The *Journ. Général* also announces the final volumes, xiv. "Quads." and x. "Poissons," in Brumaire, xi. (October 1802). Probably Saugrain dated his volumes l'an vii. because they were all printed and ready to issue by that year, but why Didot so dated his new title-pages is a mystery. Engelmann, *Bibl. Hist. Nat.* 1846, p. 322, refers to a separate issue of the "Tableaux" in 1802 in 18mo, Plassan; and among the bibliological treasures of Professor Newton is a unique copy of this, containing only the birds, which belonged to Fischer de Waldheim, and has his annotations throughout. It is repaged but otherwise identical with the corresponding part of the birds in vol. xiv. of the "Quads." in the Didot Buffon.

So much for the Didot Buffon, for much of the elucidation of which we must thank Mr. Richmond. Let us now consider the history of the "Tableaux." As stated in my previous letter, Lacepède read his paper on Mammals before the Institute on 21 Prairial An vii. (June 9, 1799), and his paper on Birds 6 Fructidor An vi. (August 23, 1798), or nearly a year before. They were published together in the *Mémoires de l'Institut*, vol. iii., in 1801. They have been frequently quoted as 1799, but no copy has ever been produced in support of the statement. I imagined I had found the 1799 issue in the Buffon, but Mr. Richmond has argued that I was mistaken. There was still another reference in Engelmann (p. 376) to an issue "in-4, *Paris, An* vii. (1799), Plassan (38 pages)." This tract I had searched for for years, and almost despaired of obtaining, when quite by chance I discovered its more exact title, made a fresh attempt, and after several months succeeded in securing a partly uncut copy. Here is the full title, "Discours | d'ouverture et de clôture | du cours | d'histoire naturelle | Donné dans le Muséum national d'Histoire naturelle, | l'an vii de la République, | et | Tableaux méthodiques | des mammifères et des oiseaux, | par le C^en^ Lacepède, | De l'Institut national de France [7 lines of titles, etc.] | a Paris | chez Plassan, Imprimeur-Libraire. | L'an vii de la Republique. | —These "Discours" occupy 55 pp., p. 56 is devoted to "Errata," and then follows "Tableau des divisions,

sous-divisions, ordres et genres des mammifères par de C^en^ Lacepède," [etc.] "A Paris, chez Plassan [etc.] L'an vii de la République," pages 1-18; followed by "Tableau des sous-classes, divisions, sous-divisions, ordres et genres des oiseaux" [no title-page or date], pages 1-20, and "de l'imprimerie de Plassan."

The whole is bound in the original boards, and is uncut in two places; is obviously a complete and perfect book; contains the 38 pp. tract referred to by Engelmann on p. 376; and was reviewed in *detail* in the *Journ. général Litt. France*, iii. 1800, Nivôse An viii. (December 21, 1799, to January 19, 1800), p. 7. Having, therefore, found this missing book, we are now in a position to speak definitely as to the date of Lacepède's generic and specific names. They first appeared in print quite towards the end of 1799, in the newly found tract. They were quoted by Daudin in his "Traité Orn." 1800 (Richmond, *Auk*, xvi. p. 327); they were printed in *Mém. de l'Inst.* iii. 1801, reset and repaged, and with the "errata" which appeared in p. 56 of the "Discours" corrected; they also appeared, with additions by Daudin, in vol. xiv. of the *Quadrupèdes* of Didot's Buffon in October 1802; from which volume separates were issued in the same year, of which one copy, and that of birds, is known. It is further to be noted, as a correction of my previous note, that of the *specific* names Lacepède supplied one each to the mammals, as seen in the original tract and in the *Mém. de l'Inst.*, while the whole of the other specific names are Daudin's, and their date must of course be a matter of separate individual inquiry.

Professor Newton, who has followed the whole inquiry in the kindest and closest manner, has carefully examined the 1799 tract and says: "My impression is that the two 'Tableaux' (Mammals and Birds), each of which has its own pagination, distinct from that of the 'Discours,' must have been printed *before* the 'Discours' were—and the Birds indeed so long before as An vi., on the 6 Fructidor, of which year the *table was shown* to and a memoir upon it read before the Institute, as he himself [Lacepède] stated on the 21 Prairial of the following year, when he presented the Mammal scheme. You will observe that the 'Tableau' of Birds in your copy has no separate title-page, and it looks as if that was printed off in An vi.—the 'Tableau' of Mammals not being printed till the following year—in which year also the little Buffon, according to its original title-page ('chez Saugrain,' etc.), was printed, *i.e.* 1799, while in my view the original 'Tableau' of Birds belongs to An vi., *i.e.* 1798! Whether *publication* in the strictest sense of the word can be claimed successfully for that date is more than I should like to say; but there ought, I think, to be no doubt that his genera both of Mammals and Birds were published in An vii. (1799)."

In a further letter Professor Newton points out, that while in the *Mém. de l'Inst.* the errata that appeared on p. 56 of the "Discours"

were corrected, they were *not* corrected in vol. xiv. of the "Quadrupèdes" of the Saugrain Buffon. And this fact seems to suggest that the seventy volumes issued by Saugrain were all printed in or before l'an vii. (1799). Why their publication was delayed, or arranged as two volumes a month, we may never know.

It follows, therefore, that—

Lacepède's "Tableaux," 1799, exist.

Their date is 1799.

All the generic names are Lacepède's.

The specific names in the original tract and in *Mém. de l'Inst.* are Lacepède's; but all the rest which appeared in the 1802 volume are Daudin's.

I have entered rather fully into this subject, and if the general reader of *Natural Science* considers the whole thing a bore, it will at least show him that much and tedious labour is necessary before one can solve so apparently simple a problem as the date of publication of a mere name, or even of an individual volume.

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An Extension of the Method of treating Variations, with Examples and certain Conclusions.

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It is proposed to give in this paper a short account of an extension of the method at present used in the study of variations. Examples will be shown in order to illustrate the working of this method, and a brief discussion of the conclusions towards which the results already obtained point will be added.[1]

It is unnecessary to enter into the details of the present method, since they are now so well known, and only the underlying principles will be mentioned in order to preserve the continuity of the subject and display the exact point of the new departure. For the sake of clearness the various stages will be denoted by propositions, three in number: (1) the application of the laws of probability; (2) a law which holds for all the individuals of a "group"; (3) a formula for determining to which of known groups any chosen individual belongs.

1. The variations of any organ or part of an organ in a series of individuals of the same race or species conform to the laws of probability. When arranged in order these variations form a curve which may be expressed by one of several algebraic equations. The most common of these equations is that known as the "Probability Integral." Further, when the variations of one organ have been expressed, a constant can be found showing the relation of these variations to those of another organ; in other words, the correlation of organs can be expressed mathematically.

With two great exceptions the examples hitherto given have been concerned with the variations of particular organs and the correlation of these variations. The conclusions have been restricted for the most part to displaying the "fact" of variation and the importance of the mathematical method. More recently an effort has been made to pass beyond this stage and connect the observed change in a range of variations at different times with a known change in the environmental conditions.

It is necessary here to enter into a slight criticism of this position

[1] For conclusions, see pp. 417 *et seq.*

in order to contrast it with the position shown later. If it is sought to connect directly the rate of variation in any one particular organ or part of an organ with a certain change in environmental conditions, it is not difficult to show that the conclusions reached will depend much more upon the pre-existing assumptions with regard to the relations of the organism and environment than upon the actually observed facts.

This can be shown mathematically. If $\phi(x)$ represent a group of organisms, *i.e.* their characters, let x_1 be one single character; $\phi(x_1)$ will then represent this character in the group and $\phi'(x_1)$ its rate of variation. Further, let $f(x)$ represent the group of environmental conditions, then $f'(x)$ is the rate of variation of this group. That $\phi(x)$ varies with $f(x)$ is the accepted position in biology.

That $\phi'(x_1) = kf'(x)$ is the above position with regard to the rate of variation in a single organ, where k is a constant depending on $f'(x)$. But x is composed of many variables, say x_1, x_2, x_3, etc., hence the true relation between the rate of variation in $\phi(x)$ and $f(x)$ must be $\phi'(x_1\, x_2\, x_3 \ldots) = kf'(x)$. These two equations cannot both be true except on two extreme probabilities: that the other organs do not vary, or that the rate of variation is the same for all. This means that $\phi'(x_2)$, $\phi'(x_3)$ . . . which include the variations due to growth and correlation, are all equal and each $= \phi'(x_1)$. This assumption is obviously a very great one, but even then we have only come to the observed fact that $\phi'(x_1) = kf'(x)$. We come now to the conclusion that k measures the rate of change of $\phi(x_1)$ with regard to $f(x)$, but what then? The meaning we give to k must obviously depend upon the assumption we make as to the relations between $\phi(x)$ and $f(x)$. In other words, k cannot be taken to prove our original assumption. There seem to be but two ways of regarding the relation between the rate of variation of an organ and a change in the environment, the one that the relation is direct, the change in the environment causing the alteration of the organ during growth; the other that the relation is indirect—the change in the environment bringing about the alteration in the organ by destroying the individuals which did not possess the actually observed altered organ. In the former case, k is like any observed constant in the science of physics; in the latter, it is a measure of natural selection.

The question then comes to be, which of these assumptions will best explain the facts? Hitherto the theory of natural selection has flourished under the belief that it could explain the facts rather than that the facts were rightly explained. In the conclusions of this paper an endeavour will be made to show how this theory rests on an assumption which, however probable in appearance, must always remain unproven, and it will be suggested that the counter-theory explains the facts better.

If it is difficult to make a just comparison of the changes in a single organ with the changes in the environment, it is equally difficult, on the other hand, to make such a comparison for the species. It is the "species" that has formed the starting-point of the theory of natural selection, and by the light of the "species" the structures of the individual, its birth, every portion of its life, and even its death, have been interpreted. But the "species" is a quantity not easy to measure, and it thus seems very wide of the mark to talk of a

character, or the variation in a character, as being "good for the species," as having a "selective value," when nothing definite is known.

On the one side, therefore, we have "organs," on the other side "species"; and when we consider things as they naturally are, over all is the environment. Is it not possible, then, to find a method of grasping the mean? Between the organs and the species lies the most real of all, the individual—the unity of biology. If we could but understand the single life in its entirety through concentrating and testing on it all the conceptions of biology, we should know better the meaning of "change" — how it arises, and thence also the meaning of "evolution."

This is the background of the task which Professor Heincke[1] set himself. After various trials at combining variations, and the making of formulas to represent groups and species, he advanced towards a method of determining, and a conception of the individual—not as an abstraction, but as something real and composed of organs, and forming one of many exactly equal under equal conditions.

The method consists, not in the correlation of the variations of two or three characters, but in the correlation of the averages of as many characters as possible. If the variations of many characters are obtained, those of each character may be arranged about the centre 0, as in the ordinary mode of dealing with variations. Hence a system of groups of variations is obtained, each group representing the most probable distribution of the variations for that character, and the common centre representing the average of each group. If the variations are then arranged in parallel columns they may be summed up and treated as if they were deviations from the common average at 0.

In other words, the sum represents the distribution of the variations just as the ordinary arrangement of deviations about an average represents their chance distribution.

The assumption underlying this method[2] is that each group of variations conforms to the same "type" of probability, that of the "probability integral." This assumption has already been challenged as if it invalidated the whole principle, but although it is not easy to say what correction should be made, a summation is certainly possible, and the warrant of its being near the truth is shown in the results.

The law which arises from this summation holds good over all the individuals of a group under the same conditions. It gives the second proposition.

2. The standard deviation of all the variations of the individuals when grouped about the common centre 0 is 1·18, and the probable

[1] "Naturgeschichte des Herings," *Abhandl. des Deutsch. Seefisch.-Vereins*, B. ii. H. 1, 1898.

[2] A further assumption is that each character presents an independent series of variations.

error 1. The proof of this proposition here given is slightly altered from that given in the original, but seems a little clearer.

Let k be the number of characters,
n the total number of variations,
d any deviation of any character,
v the corresponding variation,
w the corresponding probable error,
and N the number of variations of any character.

Then $$\frac{\text{Sum of all the variations}}{\text{Number of variations}} = \frac{1}{k}\Sigma\left(\frac{\text{Variations of any character}}{\text{Number of variations of this character}}\right)$$

$$i.e. \quad \frac{\Sigma v}{n} = \frac{1}{k}\Sigma\left(\frac{v_1+v_2+v_3\ \ldots}{N}\right)$$

$$= \frac{1}{k}\Sigma\left(\frac{\frac{d_1+d_2+d_3\ \ldots}{w}}{N}\right)$$

$$= \frac{1}{k}\Sigma\left(\frac{\frac{d_1+d_2+d_3\ \ldots}{N}}{w}\right)$$

$$= \frac{1}{k}\Sigma\left(\frac{\text{Average deviation}}{\text{Probable error}}\right)$$

but on the assumption that has been made, if the number of individuals and characters observed be increased, each fraction on the right-hand side of this equation tends to an equality, and from the probability integral—

$$\frac{\text{Average deviation}^{1}}{\text{Probable error}} = \frac{1}{\cdot 8453} = 1\cdot 18.$$

Hence, the sum of all the variations[1] divided by the number of variations is equal to 1·18, and this is the standard deviation of a curve whose centre is at 0 and whose probable error is 1.

Two examples may be given in order to illustrate this conclusion. The first is of a group of 50 herring from the White Sea, and is taken from the work of Prof. Heincke.

$\frac{\Sigma v}{n}$ for 5 characters,	differs from 1·18	by 0·40
for these + 3 more,	,, ,,	by 0·29
for 13 other characters	,, ,,	by 0·25
for all (21)	,, ,, ,,	by 0·21.

The difference between theory and observation is evidently very small. In the second example the variations of 54 plaice from St. Andrews Bay are tabulated in full according to the method. Under the second

[1] The terminology used here is that of Galton, Weldon and Pearson. "Deviations" are the observed fluctuations about the average of any character, "Variations" these deviations when expressed in terms of the probable error. These correspond with the terms used by Heincke.

heading are arranged the frequencies of the variations, under the third the average or standard deviation of the variations for each successive group of five characters, and under the fourth the probable error of each group.

Character.	Deviations[1] expressed in terms of the "Errors of Mean Squares."													Average Deviation of Variations.	Probable Error.
	−3	2·5	2	1·5	1	·5	−0+	·5	1	1·5	2	2·5	3+		
Intermaxilla (*l*)		2		9		20		14		7		2			
Intermaxilla (*a*)	3		5		14			24		4		4			
Tail (*l*) . .		1		10		15		14		9		5		1·14	·96
Eye (*l*) . .	1	1		6	12				8	6		5			
Mandible .	1		4	6	4	4		9		5	2	2			
Head (*d*) .	3		5		10				6		9		4		
Head (*b*) .	2	3	0	7	0	11		18	0	7	0	5	1		
Head (*l*) .		6			16			21		7		4		1·42	1·29
Body-Height .	2	0	4	5	7	8		4	5	0	8	5			
C. Vertebrae .	4												7		
A. Vertebrae .	5												2		
Fin-rays P(*r*) .	2				24				26				2		
„ P(*l*) .		1		8					13			2		1·33	1·12
„ A .	1	4	0	4	8	6		4	6	5	1	2	3		
„ D .	1	2	3	7	3	12		8	6	0	3	1	3		

Average deviation for all the characters 1·29
Probable error „ „ „ 1·09.

The conclusion one would draw from these two examples is that the assumption which permits theory and observation to agree so closely cannot be far from correct. The characters were not taken in any fixed order, and the results show how the observed values fluctuate about the theoretical, the first group having an average deviation lower than the theoretical value, whilst the second and third have higher. This fluctuation gives point to an important corollary founded by Prof. Heincke on the theory shown above, namely, that if one or two characters only of all the individuals of a fixed group could be examined the variations would also agree with this law. The difficulty of obtaining this fixed group of individuals similar in all respects as to age or size, place or living conditions, etc., is therefore the only drawback.

A second corollary of even greater importance is also deduced from this theory. If this formula represents the "essence" of the variations of all the individuals, the conditions being the same, it must also do so for one. The variations of each individual are equally balanced on either side of the average, that is, if the variations of all the characters of each single individual could be tabulated they would be distributed about the mean according to the above law.

[1] The signs + and − are not used in obtaining the "Average deviation."

From this theoretical conclusion we are led on to the practical outcome. If the conditions are altered we get a second group of individuals each of which conforms to the same law, but the centre of the averages is altered. Hence if any individual be chosen at random from one of these groups we should be able to tell by an examination of its characters to which group it belonged. Hence the following practical rule.

3. The sum of the squares of the variations in the characters of a certain group is a minimum for the individuals of that group.

This follows directly from the equation of the probability integral,

$$y = \frac{1}{k\sqrt{\pi}} e^{-k^2x^2}$$

the nearer y approaches the centre of the curve, or the average, the smaller x^2 becomes. This being true for all the characters, we have that Σx^2 is a minimum for the variations in the characters of a certain group. Hence, if it is desired to know to which of several known groups a certain individual belongs, it is necessary to calculate the variations of each character of the individual from the respective averages of the several groups, then find the sum of the squares of these variations, and the least sum shows the group to which the individual is most nearly allied. The more characters that are taken the more likely is the result to be right, but less characters are necessary the greater the number of individuals. The first example taken to illustrate this is one of several given by Prof. Heincke. It refers to a single specimen of the group of herring obtained from the White Sea, which had 58 vertebrae where the average was 53·6. One might think, therefore, that this individual was abnormal for this group, or belonged to quite another group. Two other groups are therefore taken, the one from the west and south-west coast of Norway (Vaarsild), which has 57·5 as the average number of vertebrae, the other from the Jutland Bank off Denmark, which has 56·6 vertebrae on the average. When other characters are considered, however, and the variations of this single individual form the averages of the three groups calculated according to the method, we find that

From the average of the	White Sea (35 characters) . .	(x^2 or) $v^2 = 3{\cdot}213$
,, ,,	Vaarsild (35 characters . . .	$v^2 = 3{\cdot}696$
,, ,,	White Sea (37 characters) . .	$v^2 = 3{\cdot}225$
,, ,,	Jutland Bank (37 characters) .	$v^2 = 3{\cdot}617$

In each case the least value shows that this individual more closely approaches to the herring of the White Sea in spite of its having a seemingly abnormal number of vertebrae. From this it follows that whilst in one character an individual may be very much above the average, it has a variation or group of variations in other characters below the average, which balance by "defect" what the first has in "excess."

The second example taken deals with the group of plaice from St. Andrews. In one respect they do not precisely conform to the conditions necessary for obtaining a "pure" group, in that both male and female are taken. The warrant for doing so is that at the size (12 inches on average) no distinct differences between the two sexes with regard to these characters can be detected. These are compared with the plaice from Grimsby and Aberdeen. The comparison is not made with the total averages of these last two groups, but only with those of two portions which are similar in all respects to one another and similar with regard to size and sex to those from St. Andrews.

It has been known[1] that the plaice of St. Andrews Bay are the young of the plaice which spawn somewhere near the Aberdeenshire coast, and it was therefore of interest to find out if this new method of research would support the testimony obtained by another.

Characters.	Averages for Aberdeen.	$\frac{\Sigma d}{n}$	$\frac{\Sigma d^2}{n}$	Averages for St. Andrews.	$\frac{\Sigma d^2}{n}$	$\frac{\Sigma d}{n}$	Averages for Grimsby.
Intermaxilla (l)	26·80	+ ·4	·16	26·40	·58	+ ·76	27·16
Intermaxilla (a)	21·17	− ·11	·278	21·70	·59	− ·01	20·92
Tail (l) . .	29·56	+ ·23	·467	28·64	2·08	+ ·74	30·89
Eye (b) . .	35·72	+ ·085	·381	36·07	1·57	+ ·49	35·83
Mandible .	46·70	+ ·194	·384	46·07	1·305	+ ·30	45·60
Head (d) .	39·09	+ ·155	·32	39·13	1·43	+ ·02	37·73
Head (b) .	48·72	+ ·01	·38	49·57	2·03	− ·46	47·20
Head (l) .	24·31	+ ·08	·37	23·76	2·06	− ·09	25·28
Body height .	65·18	+ ·16	·40	64·37	2·04	− ·23	63·00
C. Vertebrae .	30·03	+ ·14	·36	30·05	1·84	− ·22	30·07
A. Vertebrae .	12·96	+ ·13	·33	12·95	1·67	− ·19	12·94
Fin-rays P(r) .	11·53	+ ·12	·30	11·52	1·54	− ·20	11·30
,, P(l) .	10·93	+ ·095	·28	11·13	1·42	− ·20	10·91
,, A .	54·28	+ ·086	·26	54·31	1·33	− ·21	53·98
,, D .	72·67	+ ·065	·247	72·91	1·250	− ·22	72·51

St. Andrews varies by ·247 from Aberdeen, and 1·250 from Grimsby.

The characters of this table might have been specially arranged in order to show a uniform gradation of the differences which are tabulated under the headings $\frac{\Sigma d}{n}$ and $\frac{\Sigma d^2}{n}$. But they were tabulated just as they presented themselves, and the fluctuations in these differences instead of being a flaw in the principle only show how necessary it is to take more than a few characters. The small number of characters here taken, and the seemingly very slight differences between the two original groups of Grimsby and Aberdeen, make a severe test of the theory, and thus the results are the more convincing. The values of the deviations between St. Andrews and the other groups gradually

[1] Dr. T. W. Fulton: *Rep. Fish. Board for Scotland*, No. XI., 1893, Part III. p. 176.

become smaller with the addition of every new character, and would continue getting smaller if more were added, because the number n increases faster than the deviations. But the relative proportions of these deviations shown under the headings of $\frac{\Sigma d^2}{n}$ would remain almost constant, as it does from the sixth character onwards. These proportions show that the specimens from St. Andrews are several times nearer to those from Aberdeen than to those from Grimsby. Further, from the columns of the simple deviations, $\frac{\Sigma d}{n}$, we get another important conclusion. The signs of the deviations are different, and this shows that if two curves were drawn to represent the deviations along the same axis of the characters of the groups from Grimsby and Aberdeen, then the St. Andrews group would lie between.

It will have been noticed that instead of calculating the variations for each character, the simple deviations are employed. The reason for this is that when the average deviation is small, less than 1, as it is for the most of these characters, and when therefore the average deviations of both known groups are the same or nearly so, there is very little error introduced by using the deviations directly. In this case if the deviations had been expressed in terms of the probable error the results would have shown larger numbers, as in the case of Prof. Heincke's example, but the proportions between the numbers under $\frac{\Sigma d}{n}$ and $\frac{\Sigma d^2}{n}$ would have been almost the same.

We may turn now from the mathematical to the biological aspect, and however uninteresting the mathematical method may be to most biologists the ideas which it springs from and the conceptions it leads to will certainly be the reverse. Mathematical expressions for the relations between the phenomena presented by living organisms, figures or numbers for the facts of life and the changes in organs, are utterly meaningless in themselves unless the biological standpoint is carefully maintained in the foreground. And it is just in this that one of the chief merits of Heincke's position lies.

If the student of biology brings to his studies a wholesome scepticism of what has hitherto been reported true or false, and yet in spite of his scepticism still retains a strong desire to know and understand things, he will soon come to the conclusion that the manner or method of acquiring knowledge is of as much if not greater importance than the actual knowledge. The phenomena of life, we say, form the raw materials of knowledge, and yet the mind cannot grasp the complex relations and interplay of structure with structure, of organism with organism, and of those with the environment, by entering straightway into the investigation of phenomena, here, there, and everywhere. Some preconceived notions of the subject in hand, and even more, of the right attitude of the observer to the things observed, must be formed; otherwise, however unwillingly, we shall fall into one of two grave errors—either lay stress on the phenomena and pile up detail upon

detail of description and fact without law or connection, or tending too much the other way, show too much of the observer and become guilty of anthropomorphism. The latter attitude is but too prevalent amongst opponents and upholders alike of the theory of natural selection.

The attitude here advocated lies between these two. The details and facts are welcomed, but "facts" do not make true knowledge, and on the other hand, any theory which makes the observer inclined to read into the facts his own personal notions of "utility" or "advantage," for example, must be considered as too prone to misinterpret the actual phenomena. What we wish rather is the attitude of a philosopher who perceives well the facts, but holds himself aloof from opinion, and seeks some method as an intermediary and aid to interpretation. What this method should be is not difficult to conceive.

The outstanding feature in the Darwinian hypothesis—seized upon by its clerical critics at the first appearance of the *Origin of Species*—was the stress laid upon "chance." The conception therein involved was—not that anything ever happened at random or haphazard, but that the changes occurring might be conveniently so expressed. As is well known, this theory of chance has been developed more and more of recent years as a separate study, until the biologist has come to regard it with suspicion as something foreign to his own subject. And yet is there not some truth in this theory of chance? And again, have natural selectionists the prerogative of this truth?

The truth underlying this theory is not far to seek. Life, we may say, depends on many "chances." Hence, knowing the various sources of danger, we may, as if insurance agents, calculate the "chance" of a particular individual surviving to a certain age. In a deeper sense, again, if we knew the causes of variation we should be able to calculate the "chance" of the appearance of any particular variation under certain conditions. What we have before us at any time is only partially the truth, and even if we knew all that had ever occurred and understood all, we—not being omnipotent—could only state as a probability what would occur next.

This is the justification for the theory of "chance," or we should rather say of "probability."[1] Has the theory of natural selection any exclusive right to this conception?

We may judge of this more closely by following the facts grouped under the three propositions stated above. From (1) we learn that the variations in any character are naturally grouped about a certain average, and are usually distinguished as plus and minus variations from that average. If then we hold by natural selection, and maintain that *by this law* evolution may proceed by slow minute steps, so that "even a grain in the balance shall decide which shall live and which shall die,"—which variations shall we call "useful" or "favourable"? If it

[1] A mathematical description of the meaning underlying these words will be found in the "Chances of Death," by Karl Pearson.

is said that we can only tell this by the result, when the "fittest" has proved itself, when the "selection" has already taken place, this simply means that we are unwilling or unable to understand the present. In reality there cannot be two opinions. A plus variation may be "useful" to-day, "harmful" to-morrow, and similarly for a minus one.

It will be said, however, that "plus" and "minus" do not exist in nature, and that the most "useful" or most "favourable" is the mean or average condition. How then shall we understand the continual recurrence of variations from the mean? As shown by Galton, the tendency of successive generations is to produce offspring nearer the average than the parents, and hence, on the theory of the survival of the "fittest," or most "favourable" variations, we are unable to explain the presence of extremes.

Natural selection is based on (1) the rate of increase of offspring, (2) enormous destruction and "struggle," (3) survival of the best fitted to the conditions. If we let our minds run smoothly in the train of thought suggested by the form of the premises, the conclusion is inevitable. But if we inquire more closely into the "struggle" and "destruction" in any particular case, say of the eggs and young of the herring in the sea, we find ourselves obliged to consider this destruction as indiscriminating and independent of struggle, and that consequently both the "fit" and "unfit" survive; in other words, we cannot apply these latter words even "metaphorically."

If natural selection thus fails to interpret present phenomena as shown in the variations of single organs, its difficulties increase when we consider the individuals. From the study of organs we might conclude that Nature was aiming at the conservation of the average, but when we examine many organs we find that the average of one may be combined with the extremes of others. Hence greater fitness in this or that has to make up for greater unfitness here or there. In the same region the individuals of a group at the same period of life are thus equal in the combination of their characters. This has been shown to be a theoretical deduction from the first proposition, and it has been exemplified under propositions II. and III. Any conception of greater or less fitness is here completely excluded.

When we turn, however, to different regions and consider different groups of the same species we find that the average of the individuals has changed, and if we examine successive groups in successive regions we find intermediate stages of the averages. It might be thought, then, that natural selection has brought about these differences in the different regions. If so, then we must change our conception of natural selection because there is little or no "struggle" between the different groups, consequently no discriminating destruction and no "survival of the fittest."

It is not to the present purpose to criticise further the theory of natural selection, or plead for the theory of probability. This latter,

we see, can be divorced from the former, and in its making for accuracy is in truth but the expression of rationalised scepticism. Let us turn to certain of the conclusions to which Heincke has been led by employing the theory.

The variations that we find conforming to the laws of mathematics cannot be considered as the "beginnings" or "makings" of new varieties and species; they are the actual condition of affairs, the product and reflex of the varying elements of the environment. These variations in structure are found to show the same appearance year after year, and thus a similar curve of variations will be obtained year after year although the individuals examined are of different generations. Similar curves are obtained for different groups of the same species taken from different regions although the mean or average value is changed. If we look to the environment we see that the conditions there of temperature, salinity, etc., present similar curves. We are surely entitled to connect these two sets of variation and state that the one gives rise directly to the other.

The variations in organs which are of specific importance are for the most part formed in the early stages of ontogeny when the organism is plastic and sensible to the fluctuations in the environment. The range of variations is represented by the "variation coefficient," the "probable error" of the variations. This is not exactly the same for different regions, and hence forms a means of comparing the ranges of the variations in the different environments. It also represents the average capability of varying which each group possesses with regard to the particular organ, whilst the total range of deviations actually observed represents the variability for the group, and thence of the species. The importance of the conception is evident.

The individual as a combination of organs is, however, the turning-point of the position of Heincke. Reference might be made to eminent biologists who have expressed similar ideas, but perhaps Herbert Spencer comes the nearest. His conceptions of "life," the "balance of organs," "direct and indirect equilibration," are almost exactly repeated by Heincke in other words. But whereas Spencer took a broad view of the problems of evolution, and thence showed the various factors in perhaps their true perspective, Heincke has concentrated his attention on the meaning of certain carefully observed facts.

Each individual of a group is the chance combination ("permutation") of a number of possibilities, each combination being equally probable. Hence the individuals at the same stage of life, whether as larvae, young, or adults, possess an equal "balance" of the possible variations of their organs. And similarly for other groups, the destruction that occurs is a destruction of combinations of equal value with one another and with those that survive. The survivors give rise to further combinations, each within the same range of variability as before, and each equally probable, the exact combination and balance

depending upon the immediately surrounding conditions. Thus, whilst each new individual represents a fresh combination of the "possibilities," the mingling of the sexual elements is the "dissolution of the chance." Death is not the giving place to the "more fit," nor the resignation of the individual for the good of the species, but the natural ending that comes to all by whatsoever "chance."

"Utility should therefore be replaced by probability." The individuals of the race start out on life with the same opportunities and capabilities, and the probability of the so-called "success in life" is inversely proportional to the "chances of death." By man's agency these latter may be and are continually being altered, and thus give rise to appearances which have formed the foundation-stone of natural selection. But apart from man it is permissible to conclude from the balance of things that the chances of success in life are in exact inverse proportion to the chances of death. If the conditions of life were continually recurring, therefore, with periodic regularity, the chances would be practically constant and the "balance" of the individuals would remain the same. But when the conditions of life change and the change remains constant the balance of the organs in the individuals is altered. Thus, whilst the adults may or may not be affected—for the chances of death may be increased or diminished—the combination of organs in the youngest must alter, and from this directly-caused change new races will arise.

A fine distinction, but an important one, should be noted here. Darwin observed that species the most widespread and most abundant varied the most. This, however, does not truly represent the case. As we pass over the different regions inhabited by the species we see that the diversity in characters is greater than in a smaller species. In reality the "variation," *i.e.* the variation-coefficient in the individuals of the different regions, may be exactly the same for all, where that which is characteristic of the larger species is its greater "variability."

From this aspect of variability we can understand how the individuals of a group differ from one another in all parts of the body and at all stages of development. No two individuals are "alike," though all are "normal" and equally "fit." This variability shows itself not merely in the earliest stages, but through every stage of development for many characters. Hence we get differences due to growth or age, the "balance" of the different organs not remaining the same throughout.

From these fundamental positions Professor Heincke passes to a criticism of systems of classification, and to suggestions for a new and better one. The older systems have begun with orders and classes and worked down towards species and varieties. This method has succeeded for the orders, but not for the species. Hence we must begin at the other end, below the "species," and work up to the orders.

The first group, therefore, is the Race or "Stem" ("Family").

This includes "those individuals which live within a certain region, under equal conditions, have the same habits, and stand in close blood-relationship through intercrossing and reproduction." The characters of these individuals will come under the laws formulated here as propositions II. and III.

The older systems regarded the above as the species, but in a natural system the "species" is the second group. It is a certain "combination" of races whose exact limitation has to be determined in all particular cases. The species is but a larger "race," and may be sharply marked off from or merge into the races of another species. The sharper the races can be marked off from one another the clearer will be the distinction between the species.

As this paper is so short, it is hardly necessary to make any summary. The endeavour has been to display the meaning and importance of Heincke's work, and if further information is desired reference should be made to the original work.

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A Zoologist on the Principles of Science.[1]

By F. A. Bather, M.A.

Professor Brooks entitles his book "The Foundations of Zoology," but he ends by referring to it, more justly, as "my work on the Principles of Science." It is not modesty that selects the less comprehensive title. It is desire to emphasise the belief "that the principles of science, as distinguished from the concrete sciences, are part of biology."

The ground taken is that the methods of extending knowledge, as well as the generalisations therefrom, are operations of the human mind. Every physical science rests on a metaphysical basis, which has its origin, so far as we are concerned, in the mind. It is the student of animals who has to face the problems presented by the origin of the mind; and it follows that, when questions are raised concerning the operations of the mind in scientific study, "the zoologist has a peculiar right to ask answers, in addition to the right which he shares with other students of science." Or, from another point of view, since life is response to the order of nature, the study of the order to which response is made is as much a part of biological study as is the organism which responds.

When I found this to be the author's conception of his office, a sense of incompetence urged me to withdraw from the attempt to deal with matters so profound. But the description of the book as "a course of lectures delivered at Columbia University" reassured me: at least I was capable of learning. We students of zoology need a book to show us the relations of our science to broader schemes of philosophy, a book written from our own standpoint and condescending to our ignorance. The fact that this need is not recognised by all of us merely shows how real a need it is. And here, perhaps, lies the chief value of the present work. Dr. Brooks has so deserved a reputation as a zoologist, that any writings by him on "the foundations of zoology" are sure to be read by his fellow-workers; and through these essays their interest will be stimulated and their intellectual sympathy widened. But as "a course of lectures"! Well, the least

[1] "The Foundations of Zoology," by William Keith Brooks. Columbia University Biological Series, vol. v., 8vo, pp. viii. + 340. New York: The Macmillan Co. London: Macmillan and Co., Ltd., 1899. Price 10s. 6d. net.

that can be said is that the students of Columbia University have not been fairly treated. Some old lectures "prepared at different times and for various reasons" have been furbished up and intermingled with extracts from reviews and other magazine articles. The almost unavoidable consequence is superabundant repetition, not always free from inconsistency, a want of coherence, not wholly remedied by an interjected paragraph or two, an absence of logical arrangement and continuity in the development of the main thesis, and long complicated sentences to be attacked only by the midnight reader with a wet towel.

Despite these defects, the conclusions or leading ideas of the book, if not simple, are few. In fact the author states that his sole purpose is to show that mechanical conceptions of life and mind cannot make right deductions from true principles untenable (p. 29). This statement, however, scarcely illustrates the scope of the work, and the reader will doubtless wish to know what those particular deductions may be that Dr. Brooks holds to be proof against all attack. I shall therefore attempt a brief relation of the leading ideas in the book.

The two fundamental conceptions that appeal specially to the biologist, and are in large measure the outcome of his labours, are the principle of genetic continuity and the principle of fitness. Significant resemblances recognised between the phenomena of nature may be due to genetic continuity; and the order of nature may be the order of fitness.

The meaning attached to fitness by Dr. Brooks is at once seen in the second Lecture, entitled "Huxley, and the problem of the Naturalist." It is mainly a criticism of Huxley's essay on "The Physical Basis of Life," and its keynote may be thus expressed.—Admitting that protoplasm is the physical basis of life, and even supposing that its properties are a result of its molecular structure, still life is not one of those properties, but the adjustment of the properties to the environment, so as to promote the welfare of the species. As Aristotle put it, the essence of a living being is not what it is made of or what it does, but why it does it. The problem of the naturalist is therefore the study of this adjustment; in other words, the problem of fitness (p. 39). Later on, however (p. 246), we are told that "the problem of the naturalist is not the existence of adaptations as such, but the existence of adaptive species." The limitation will be found important.

The problem stated, we proceed to its consideration; and the next three lectures deal with one of the proposed solutions, that of Lamarck, and the so-called Neo-Lamarckian emendation of it. "Stated briefly," and, I think, fairly, "it is the doctrine that organic evolution has been brought about, or at least greatly aided, by the inheritance of nurture." By "nurture" we are to understand all manner of modification due to the external world.

To this doctrine Professor Brooks raises an objection that seems to have an insecure foundation. We all admit a present fitness in the

organic world, by no means an absolute fitness, but enough to call for explanation. We also admit that living individuals are capable of nurture. But, says our author, the nurture may be good or bad, quite as often the latter as the former. Therefore "the view that nature is inherited nurture throws no light on the problem of fitness." Accepting the premises, and setting aside natural selection and other factors, we might grant that unfit modifications would so counterbalance fit modifications that the conclusion would follow. All turns on what is meant by good and bad nurture. Nurture is, broadly speaking, the influence of environment on the individual. Now environment can be called favourable or unfavourable only from a relative standpoint; that is, so far as the individual is or is not adapted to it. We know no absolute good, no ultimate morality. As Dr. Brooks elsewhere says, "no natural response can be beneficial under all circumstances"; education and experience (which, be it noted, are forms of nurture) enable organisms to distinguish the harmful from the beneficial occasions (p. 13). It is admitted that we start with individuals fairly adapted to their environment, and that change in the individual or the race is induced by change of environment. But it is clear that any change of environment breaks the harmony and must be unfavourable to the individual: natural actions are beneficial only "so far as the environment is, on the average, like the ancestral environment" (p. 10). The change continues unfavourable until the individual or the race is modified in accordance with it; but this modification is itself beneficial only so long as the same change persists or continues in the same direction. Nurture is found to have been "bad," when the change of environment has been only temporary or extraordinary. Man, subject as he is to so many and great changes of environment, is often led into surroundings or habits at variance with the general conditions that govern the existence of his race: these things we rightly call "bad." But with other organisms and in physical nature changes of environment are, as a rule, secular, and proceed equably in a certain direction. Therefore their action on individuals is regular; in other words, the nurture is "good" on the whole. But if it be conceded that the good preponderates ever so little over the bad, the objection of Dr. Brooks becomes invalid.

Other considerations advanced by our author may render the Lamarckian doctrine unnecessary or less probable; but I fail to see that they prove the inheritance of modifications to be either impossible or ineffectual.

The first consideration is the truth of natural selection and its adequacy to account for animated nature as we see it. Most Neo-Lamarckians admit natural selection, though not its complete adequacy. But even that might be admitted without diminishing the adequacy or effect of any other factor. It is hardly necessary to point out that, under any theory of heredity and development, the rate of progress

through selection will be proportional to the number of variations in the direction of progress. If variations be governed by the laws of chance alone, that number must be less than when variations are determined in the direction of the environment by the inheritance of modification. Fitness would be reached more readily if modifications were inherited.

The second consideration is more subtle. It is a question whether education or the action of the external world can add anything to the nature of the organism, whether it does not merely unfold and develop the original nature. Here is the old difference between development by epigenesis and by evolution. Dr. Brooks makes a compromise that seems consistent with common sense. The organism, he says, would not develop without the education, but the character of the development is due to its original nature (p. 15). No vital action takes place without a stimulus; but the stimulus is one thing, the character of the action is another, and is dependent on the nature of the organism. Thus in ontogeny each change may be called forth by some mechanical stimulus, either within the body or without, and yet the nature of the whole may depend on the nature of the germ (p. 59). "External conditions press the button, but it takes all the inherent potency of living matter to do the rest" (p. 61).

An ingenious application of this conception may be noted in passing. It is that "organs once adjusted to the external world may, after the adjustment has lost its meaning, be still retained, because they furnish physiological stimuli, which excite developmental changes in the organic mechanism" (p. 10). Thus Dr. Brooks accounts for the retention of so-called rudimentary organs and recapitulatory stages.

But we have to see how the conception affects the problem of fitness. If it be correct, if, in other words, nurture adds nothing to nature, then there is nothing to be inherited. But the problem does not become easier of solution. It consists of two parts: the adaptability of the individual; and the adaptation of the race. The adaptability of the individual resolves itself into the adaptability of protoplasm, and none is so bold as to say he knows the explanation of this. Turning to the adaptation of the race; each new germ would, on this conception, be similar in all respects to the primordial protoplasm, being in fact nothing but an extended part thereof, but gradually becoming more and more gifted with the power of growing into a being modified in accordance with its environment. But it is very difficult to see why or how it should obtain this power, except through education. The faculty of being educated was, we may suppose, present in the original protoplasm; and it has gone on being educated ever since. Some portions of it, from one cause or another, did not respond so readily to education, and they have been expelled in consequence; that is what we mean by natural selection.

True it is that this way of looking at the case brings in the direct action of the environment just as much as ever, and that everything depends on this and on the fundamental properties of protoplasm, or, if you will, living protoplasm (we know no other). Thus, by accepting the contentions of Dr. Brooks, I am led to the very position he is trying to attack.

Here seems the place to allude to two passages much further on in the book (p. 187). "A living thing is a being which responds to the stimulus of one event in such a way as to adjust its actions to other events of which the stimulus is the sign, and as all that have not thus responded have been exterminated in the struggle for existence, the adjustment of the survivors is no more than might have been expected." "They who assert that it [natural selection] is inadequate because it fails to show why beneficial response should ever follow a stimulus, and thus furnish fitness to be selected, must remember that all science is inadequate to exactly the same degree."

These sentences seem to imply that the fitnesses or adjustments selected are the outcome of response to a stimulus, and not merely response, but beneficial response, *i.e.* response in harmony with the environment. We do not know and need not know the why or how of this responsive faculty; it is to be enough for us that it is a property of living things. It is not clear how this differs from the following statement by another author:—"All adaptations, at any rate all adjustments concerning whose action and efficacy there is no dispute, have arisen in the same way as the enlargement of a muscle by exercise," *i.e.* as beneficial response to a stimulus; and this faculty of response is "a fundamental property of protoplasm" (Cunningham, *Nat. Sci.* vol. viii. pp. 328 and 330; May 1896). But there must be a difference, for these are assertions which Dr. Brooks combats with abundance of sarcasm.

Perhaps the explanation is that we have here "a bad and unapt formation of words." "Adjustment" seems to be used in two senses: the act of adjusting and the result of adjusting. Just so Professor Brooks sometimes uses "*nurture* instead of *acquired characters*," whereas the latter are elsewhere more correctly spoken of as "the effects of nurture" (pp. 55 and 172). Since Dr. Brooks and the Neo-Lamarckian both admit (I believe) the operation of natural selection, the difference between them seems to lie in this: that, according to Dr. Brooks, the faculty of adjusting is a character that varies and is selected and inherited; while, according to the Neo-Lamarckian, the results of adjusting are the characters that are selected and inherited. It is, however, clear that selection can act on the faculty of adjusting, only through its concrete results. Further, no human being can perceive whether the faculty of adjusting is transmitted, except by seeing the results. But in the ovum these results will not be manifest to the most keen-eyed microscopist; like all other characters, they will appear

gradually. Who then shall decide whether they are the results of adjustment *de novo* in each case, or whether they are the inherited results of prior adjustment? So far the history of the controversy has shown every test-case to be capable of two interpretations.

In Lecture IV., "Lamarck," a third consideration is brought forward. Dr. Brooks tries to show that inheritance of modifications, even if admitted, would not produce such a world as we know. This he does by citing a number of instances in which the modifications affect other species (*e.g.* the bee's sting, the serpent's poison), or other individuals than the ones exhibiting them (*e.g.* the rabbit's white tail); also modifications for the good of the species, occurring only in non-reproductive individuals, and therefore incapable of inheritance. He maintains that "*in all cases* the structure, habits, instincts, and faculties of living things are primarily for the good of other individuals than the ones that manifest them" (p. 88); "there is nothing anomalous or exceptional" in the instances which he selects. This does not mean that the serpent's tooth is useful to the rabbit, or that bees sting us for our moral edification. None the less it is a hard saying, and difficult of application to such protective structures as the carapace of the tortoise, or to such (apparently) useless characters as baldness.

In the vast majority of instances this "general law" can be nothing else than that an organism has such structure, faculties, etc., as enable it to produce offspring. But we are told, *every* character is *primarily* for the good of others. Senile characters, which, as in the Ammonites, appear ever earlier in succeeding generations, may be explained as due to the direct action of the environment, or perhaps in some roundabout way by natural selection. But imagination boggles at the idea that they were of use to offspring born long before the characters appeared.

The deeply interesting Lecture V. adduces migration as instance of an action for the preservation of the species, but often leading to the loss of the individual, *i.e.* an action for the good of others, and therefore not explicable on Lamarckian principles. But though natural selection be admitted, no multiplication of similar instances can disprove the operation of the Lamarckian factor.

Lecture VI. attacks the evolutionist philosopher, he who holds not merely that the universe has evolved, but that its evolution in that particular way was a necessity from the beginning, and that all was latent and determined in the primal nebula. Not that this philosophy may not be correct, but that it is, as Huxley said, premature.

A note shows the fallacy of Galton's and Weismann's view that the ancestors of an individual are doubled for each generation that one traces them back. The fallacy lies in the omission to recognise the almost inevitable inter-breeding.

Lecture VII. continues the criticism of Galton. His data "fail to prove that the 'principle of organic stability' owes its existence to anything except past selection; that regression to mediocrity occurs

when *ancestry* is studied uncomplicated by *nurture;* that the 'mid-parent' is anything else than the actual parent; that 'sports' are fundamentally different from the ordinary differences between individuals; or that natural selection is restricted to the preservation of sports" (p. 178). Galton's statistics are no evidence as to the effects of inheritance, because inheritance is of many characters, not of one only; and in the statistics the effects of nurture are not sifted out.

Lecture VIII. defends the pure natural selection theory of Darwin against two criticisms.

First, that, since natural selection "does not produce, but only preserves the fitness which exists, it does not show why there should be any fit to survive, but only why the unfit are exterminated." Dr. Brooks says "the statement that selection could not act unless they [the useful variations] existed is childish" (p. 184). Why? Because "it is obvious." Very well! then it is equally obvious that natural selection does *not* "account for the whole history of" any character Granting variations, natural selection is an adequate explanation of the origin of species. But this is not the same as the evolution of our present fauna and flora from a protoplasmic slime. Childish though our curiosity may be, we cannot close our minds to the questions: Why does living matter vary? how is it that variations are inherited? is there a limit to variation other than that imposed by physical conditions? with many other questions that have nothing to do with the operation of sorting into species, but which call for answer before we can understand the mode of organic evolution. Because Darwin, as Dr. Brooks justly urges (p. 187), wrote for a certain set of readers and on a certain problem, this is no reason why we are never to proceed beyond that problem.

The second criticism is that many differences can have no selective value in their incipient stages, whether of organic structure, as instanced by Mivart, or of mental action, as instanced by Romanes. This objection can only be met by dealing with each instance in turn, and showing that it has selective value.

Lecture IX., "Natural Selection and the Antiquity of Life," appeared in the *Journal of Geology*, and was dealt with in *Natural Science* for October 1894.

The remainder of the book considers the evidence for purpose in nature, and first the argument from design as given by Paley. This is stated in two forms. First (p. 258), "(1) Nothing accounts for watches but mind. (2) Nothing accounts for living things unless it accounts for watches. (3) Nothing but mind accounts for living things." This is most obscure, and the phrase "accounts for" is ambiguous. If it means "is the sole cause of," then the minor premise begs the question or else is absurd. If we interpret (2) as, "The cause of living things involves the cause of watches," which is true, then the only conclusion is, "The cause of living things involves mind," even as it includes

many other things, which are the results of life, not its cause. The second statement of Paley's argument has for its major premise "Evidence of usefulness is evidence of design." But this also is just what has to be proved, and it certainly cannot be laid down as a universal proposition.

But it is late in the day to be discussing the logic of the argument from design, and it is only for sake of reference that I venture on a more complete statement:—(1) Watches and the like are admirably fitted for a useful purpose. (2) They *are* the result of design. Therefore (3) other things admirably fitted for a useful purpose are *probably* the result of design. (4) The structure of living things is so fitted. (5) Therefore they are evidence of design. Design in every case is understood to imply a designing mind. This cannot carry conviction. Premise (2) is an induction liable to be upset by a new fact. Proposition (3) is an argument from analogy only. Premise (4) can only be proved by the accumulation of instances.

The argument, in short, is one of probabilities, and the important question is how far those are affected by the acceptance of natural selection.

Dr. Brooks rightly remarks that "the mere extension of the domain of natural causation," "the demonstration of the mutability of species," in a word, evolution by descent, cannot give a blow to the argument. The fitness is not thereby affected, and the conception of design appears even more necessary.

Nor does the substitution of physical causes in place of special creation weaken the inference. The fitness remains, however brought about; and there is no reason why the designer should not work through physical causes.

Huxley is represented by Dr. Brooks as saying that there is a wider teleology which is untouched by natural selection; but his words are—"not touched by the doctrine of evolution"—a very different matter, as Huxley always insisted. What he did say about the Darwinian theory was that it was absolutely "opposed to teleology as it is commonly understood." Since the commoner teleologists agreed with Huxley in this opinion at least, it is fruitless for Dr. Brooks to raise objections. How far the more subtle teleologists may be affected is another matter.

The new aspects of the case introduced by admitting the all-potency of natural selection seem, put baldly, to be these:—Our proposition (4) ceases to be true; since natural selection implies that for every individual which is fit and persists, a hundred or a thousand are unfit and perish. This is a different idea from the absence of perfection in those selected, which would not invalidate the argument from design. Applying the teleologist's favourite analogy, the present point may be enforced in two ways, thus: that a designer 99·9 per cent of whose plans are rejected has chosen a wrong profession; or that if of a

thousand stones one happens to fit a hole in the wall, this is no proof that it was shaped with that hole in view.

Next, the probability of proposition (3) is lessened. The argument from analogy loses much of its force. Watches may be good or bad, but from the beginning they have been made with the express design of measuring the divisions of the day; to reach this result, obstacles are overcome and portions of the physical universe bent to the will of the designer. On the theory of natural selection, the evolving species presents none of these features; we deceive ourselves when we see in the Palaeozoic brachiopod signs of the direction in which its Mesozoic descendants will evolve; obstacles if presented are not overcome, but cause the line of evolution to swerve; no part of the physical environment is controlled by the species to its good, but the history of the species is controlled by the environment.

The argument from design, as stated by the older teleologists, seems to be seriously weakened by the theory of natural selection as stated by Darwin. On the other hand, that same theory may, as Dr. Brooks shows, enable us to restate the argument in a more convincing manner. First it is to be noted that all human contrivances are subject to natural selection in the same way as are the contrivances of other animals and as the animals themselves. This strengthens the analogy, but does not render it anything other than analogy; for liability to natural selection is no proof of similarity in other respects. Natural selection, to adopt a phrase dear to Dr. Brooks, shows how things happen, but it does not show why they happen.

Further consideration reveals a more fundamental change in our ideas. "The modern zoologist," says Dr. Brooks, "must ask whether we are sure that nothing but mind accounts for watches" (p. 259). "The progress of zoology has forced us to ask anew the old question whether a watch may not be part of the chain of physical causation just as truly as the spider's web or the cat. . . . The discovery of natural selection has put the matter in a new light" (p. 264). If the suggestion be admitted, "Paley's analogy" does indeed "become impregnable," but his major premise (that mind is the cause) becomes the proposition to be proved.

The problem remains, but must be attacked in another way. Whatever be the explanation of the phenomena of life, there is no doubt as to the usefulness of those phenomena to the living beings that exhibit them; and in this lies an apparent distinction between living and not living things. This faculty of using or controlling portions of the physical world, a faculty which Dr. Brooks chooses to express by the word "contrivance," may be regarded as "interference with the order of physical nature" (p. 273). Without following Dr. Brooks in his discussions of personal identity and spontaneous generation, we may agree to the continuity of life, and must admit that this faculty is coextensive with life. Now natural selection shows us the

"chain of physical causation which joins the works of man and of other living beings to that part of the order of nature to which they are adjusted." The remaining question is whether life itself and all its faculties are purely physical phenomena, or contain something which cannot "be expressed in terms of physical matter and mechanical energy." If purely physical, then usefulness and contrivance are a part of, and not interference with, the order of physical nature; and living beings can "afford no peculiar evidence of purpose." But the question is not yet answered, and the argument from contrivance, though its probability is vastly lessened, has not yet received its death-blow.

But, supposing the mechanical conception of life to be established, and admitting that the argument from contrivance would thereby lose its force, the attempted proof of the existence of a designer would not on that account be supplanted by disproof. Further, whatever the scientific account of nature may ultimately be, it can throw no light upon the primal cause or final purpose of the whole or of any part. Science tells us what takes place, and how it takes place, she discovers the succession of events and gives us a reasonable confidence in the steadfastness of that succession, but she refuses to admit any necessity therefor, and as to any cause that lies behind the veil of the physical universe, she remains for ever dumb.

But, though the scientific method may throw no light on anything beyond the facts of nature, it is still open to inquiry whether the consideration of nature as a whole may not throw some light upon the ultimate cause. Thus we are led to the conception finely expressed by Oerstedt in the phrase: "The works of nature are the thoughts of God." This view has been elaborated by a great philosopher, Bishop Berkeley, and by a great naturalist, Louis Agassiz. Each in his way maintains that the phenomena of nature constitute "a *language* in which the Creator tells us the story of creation for our delight and instruction and advantage." But each weakened his case and lost the adhesion of modern naturalists, because, as Dr. Brooks insists, he attempted to prove too much. Agassiz thought it necessary to show that the laws of nature were *nothing but* categories of thought, that they were arbitrary, and that no physical explanation of them was possible. Berkeley wrote: "The great Mover and Author of nature constantly explaineth Himself to the eyes of men by the sensible intervention of arbitrary signs, which have no similitude or necessary connection with the things signified." And in another place he held that this language of nature was necessary to assist the governed. But the modern naturalist is aware of many a physical explanation unknown to Agassiz; he sees more connection between the sign and the thing signified than was possible for Berkeley; and he refuses to admit any necessity in the matter.

But a conception supported by indefensible arguments is not

necessarily false. "As I understand Agassiz," says Dr. Brooks, "it is not because natural history is a language that he holds it to be intended; but because it is delightful to listen to the language of nature, and because it abounds in beneficial instruction for mankind." And again: "As I understand Berkeley, it is not because nature is orderly, but because the order of nature is useful, and instructive, and full of delights for living things, that he holds it to be a language." Let us admit that response to nature and the study of nature are all these things; it does not therefore follow that the language is necessary or unnecessary, and I do not see how it follows that the language is intended. It may be so, but, on the other hand, "the modern zoologist must also ask whether natural selection, so far as it accounts for living things and their works and ways, does not in the same measure account for language; both that which men use among themselves and that which we find in nature" (p. 337).

We close the book, then, as ignorant of fundamental truths as when we opened it. But we have now reasons for our ignorance. Professor Brooks, in so far as he has adhered to his maxim—"The assertion that outstrips evidence is a crime"—has convinced us of his main thesis, which indeed is a corollary of that statement, and may be expressed in the words on the wrapper of this Review:

Nunquam aliud natura, aliud sapientia dicit.

British Museum
(Natural History),
London, S.W.

On the Multinuclear Cells of some Grasses.

By Rudolf Beer.

Plates I. and II.

A few years ago we unhesitatingly affirmed that the cell was the ultimate unit of the animal or vegetable body. At the present day we do indeed continue to uphold the cell as the elementary structural component of the living body, but not without some misgivings, since recent research has made us acquainted with various phenomena which we find it difficult to reconcile with this conception.[1]

The work of a large band of investigators has shown that in a number of tissues, both animal and vegetable, the intercellular wall forms by no means so sharp a separation between the protoplasmic bodies as was formerly believed. In these cases the refined methods of modern research have revealed to us a system of delicate fibrils of protoplasm which pass through perforations in the cell-wall, and directly connect neighbouring cell-bodies.

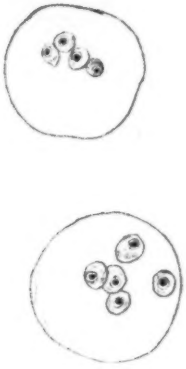
Further cause for uncertainty has been given by the discovery of multinuclear cells, *i.e.* cell-cavities which contain a single protoplasmic body in which are included a plurality of nuclei. Among the lower plants we meet with whole groups of organisms (*e.g.* Siphoneae) in which the body shows no septation into cells, although it is frequently both large and highly differentiated.

To use Sachs' phrase, the bodies of these plants are "non-cellular," for they contain a large, continuous mass of protoplasm which is studded with innumerable nuclei (15[2] and 16).

The embryo-sac of the higher plants, at one time of its existence, contains a large number of nuclei (8); the laticiferous tubes of a number of plants (Euphorbieae, etc.) are also multinuclear (18); the elongated bast-cells (18), the cells of the suspensor of some Leguminosae (6), the older internode cells of Characeae (10), the older parenchyma

[1] A full explanation of the terms used in this paper will be found in two articles, by Mr. Hill and myself, which were published in previous numbers of this journal (7 and 1).

[2] These numerals refer to papers, etc., quoted at end of article.



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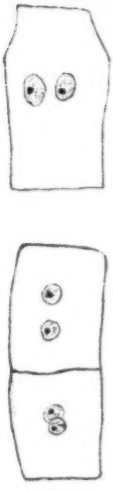

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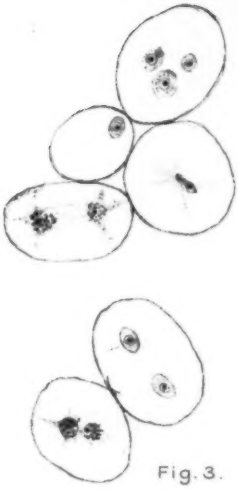

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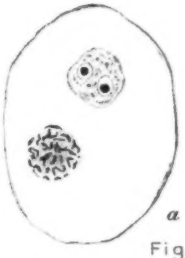

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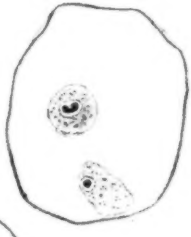

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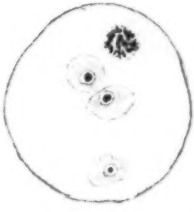

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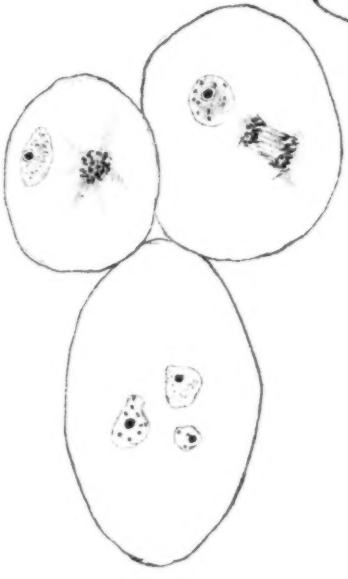

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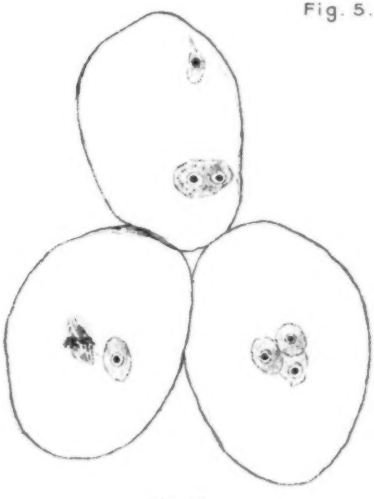

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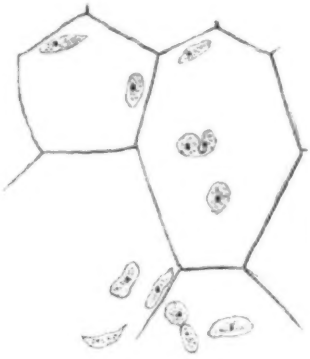

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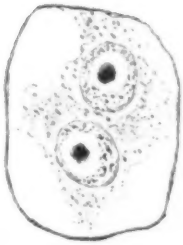

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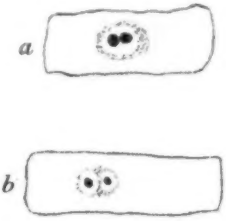

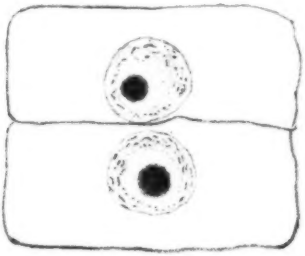

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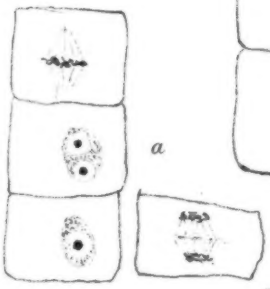

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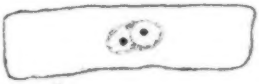

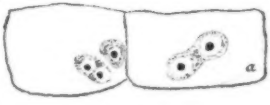

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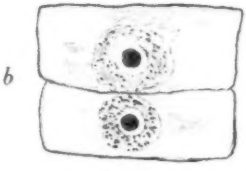

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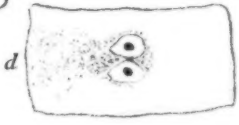

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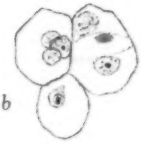

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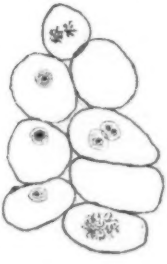

Fig. 14.

cells of many Monocotyledons (9), the tapetal cells in the sporangia of *Angiopteris* (4), the generative cells of the vessels in Dioscoreaceae (3), the older parenchyma cells of *Taraxacum officinale* (15), the large parenchyma cells of *Cereus multangularis* (18), the young, elongated pith-cells of *Ochrosia coccinea* (18) have all been shown, by various observers, to be furnished with a plurality of nuclei.

All our experience teaches us that wherever a number of nuclei appear (whether these be sooner or later separated by a cell-wall, or remain together in a multinuclear cell), they arise from the division of an original mother-nucleus. When a nucleus divides into two daughter-nuclei, it does so by one of two ways.

Either it becomes constricted here or there, and without more ado breaks into two or more parts, or it first passes through a complicated series of preparatory stages in which certain of its internal parts describe the most curious "figures," and then only separates into two daughter-nuclei. In the former case the division is said to be *direct* or simple fragmentation, in the latter it is described as *indirect* or karyokinetic (7). It is generally supposed that a nucleus which is fragmenting has lost the power of dividing activity by karyokinesis. The great German cytologist, Strasburger, writing in 1880, says: "According to my entire experience karyokinetic division and fragmentation cannot be brought together, and *certainly one cannot replace the other*" (17).

Recently, however, the Italian observer, Buscalioni (2), has shown that this separation of the two forms of division is by no means necessarily the case, and that in the development of the embryo-sac of *Vicia Faba, Lupinus, Fritillaria imperialis,* and *Leucojum vernum,* and in the laticiferous tubes of *Urtica,* fragmentation and karyokinesis may take place side by side with one another, or the same nucleus may first divide directly and then indirectly. Moreover, both Buscalioni and Dixon (5), as well as Miss Sargent (14), have observed a curious condition of the nucleus in which some of the preparatory stages of karyokinesis are gone through, but before the process is complete the nucleus divides directly. Whether this is really an intermediate stage pointing to the fundamental identity of the two processes, as the authors apparently suppose, is doubtful. The facts clearly indicate, however, that the two varieties of division are by no means incompatible with one another (19).

Some observations which I recently made on certain vegetative cells of some Gramineae give additional support to this view.

It does not seem to be as widely known among botanists as it should be that in certain members of the Gramineae, especially in *Zea Mays* (Indian corn), multinuclear cells of the most pronounced character are of frequent occurrence.

If a section be made through the stem-region of a young plant so as to pass through the enveloping leaf-sheaths, the parenchyma cells of

the foliar bases will give most instructive illustrations of nuclear multiplication unaccompanied by cell-division. In the younger leaf-bases each cell contains a single nucleus which is a well-defined, generally spherical body that stains very feebly, except the large and conspicuous nucleolus, which is its most striking feature. As successively older sheaths are examined it will be found that the nuclei increase in size, the nucleoli keeping pace with the general growth. Moreover, the clear, granule-free space, the "Hof" of Rosen (12 and 13), which is clearly seen surrounding the nucleolus, becomes broader and more noticeable with advancing age. The next stage is that in which the nuclei have undergone fragmentation, and several, sometimes five or more, nuclei can be seen in one cell. The direct method is, however, not the only way in which the plurality of nuclei originate within these cells, for I have observed quite a large number of cases in which the multinuclear condition was either partly or entirely due to karyokinetic division. The mother-nucleus of the young cell may undergo karyokinetic division, and by that means give rise to several nuclei in one cell, whilst the neighbouring cells may attain the same end by nuclear fragmentation.

Again, within one cell which contains a number of nuclei one nucleus may show karyokinetic figures, whilst another is as clearly fragmenting. Moreover, out of a group of nuclei, which I have reason to think originated by direct division, some may proceed to divide further by karyokinesis. These cases are of considerable interest as they plainly show that the nuclei, which are in a condition for fragmentation, have by no means necessarily lost their power for active, karyokinetic division.

In the older leaf-bases of *Zea Mays direct* nuclear division of a somewhat different order seems to prevail exclusively.

In the fragmentation of both the younger and the older nuclei the same impulse to divide seems to underlie the process, but the manner in which it acts differs in the two cases.

In order to understand this difference, and since the process in the younger cells of *Zea Mays* does not seem to conform in all respects to the usual descriptions of fragmentation, I may perhaps be excused for touching on this subject at greater length. In what follows I will rely chiefly on the observations which I have made on longitudinal sections of the growing-point of the root of *Zea Mays*, since the steps can here be followed with especial clearness. The same observations can (with greater difficulty, however) be made on the nuclei of the leaf-sheaths. Multinuclear cells occur, but more sparingly distributed than in the foliar organs, in the growing point of the root. The resting nucleus is a spherical or oval body which stains feebly except in the large nucleolus. This nucleolus may in some cases attain an enormous size, as for instance in the cells which are the precursors of a vessel, it usually has a perfectly homogeneous appear-

ance, but in the dermatogen cells vacuoles may be detected in its interior, it is immediately surrounded by a more or less broad investment of perfectly clear substance, the "Hof," which is sharply marked off from the granular, peripheral nuclear substance.

The first step in the division of such a nucleus is the division of the nucleolus. Immediately after division the two nucleoli lie in one "Hof." In the next stage that can be found each nucleolus is surrounded by its own "Hof." This clear belt grows more and more pronounced whilst the granular nuclear substance is gradually encroached upon and finally forms only a peripheral investment to the clear balls of substance which surround the nucleoli. The thin layer of granular substance that lies between the two clear areas appears to disintegrate or at any rate to separate *without any previous constriction*, and the two spheres of clear substance, each containing a nucleolus, are separated from one another and form the daughter nuclei. The clear body-substance of these daughter nuclei becomes later more granular, and the nuclei may move some distance apart.

Observations show that it is not merely a question of the disintegration of the granular layer between the clear areas, but that there is an actual strain pulling the nuclei in two.

It should here be mentioned that every nucleus which is provided with several nucleoli must not necessarily be regarded as indicating a stage of nuclear fragmentation, although by far the majority of resting nuclei have, in *Zea*, only a single nucleolus each.

When older leaf-sheaths of *Zea Mays* are examined, nuclear fragmentations are seen, which differ considerably from those which have been mentioned above, and conform much more nearly to the stages which have been described in other plants by previous observers.

The first and most obvious difference between direct nuclear division in the younger and older cells of *Zea* is that whilst the former takes place without the appearance of constrictions and changes of form in the nucleus, the latter is conspicuously marked by the grotesque intermediate shapes which that body assumes.

The entire absence of constricting nuclei in the young leaf-bases, which are obviously developing multinuclear cells, is the most characteristic and at first the most puzzling feature about them. Another point in which the older nuclei differ from the younger is that in the former no constant relation between fragmentation and a preceding nucleolar division can be made out. Sometimes two or more nucleoli appear here also, and one goes to each fragmentation product, but just as often one nucleolus alone is present throughout the process.

These differences, which at a first glance are so striking, are possibly associated with the alteration in constitution which the nucleus suffers with advancing age. The young nucleus is large, and has every appearance of being rich in water; the nucleus of the older

cells is a smaller, shrunken, and more solid body, provided with a comparatively small nucleolus. One can well understand how those internal differentiations, which we have described for the young nucleus, could not readily take place in the dense body of the more aged nucleus. In consequence there would be no line of weakness formed which would sharply and cleanly break across under the influence of the tension impelling division; instead, the dense nuclear substance would be drawn out and variously contorted at the line of division.

The nuclear differentiation, accompanied by a constriction which is so seldom found, marks the link between the fragmentation in the younger and older cells. In this case the nuclear substance, whilst not being too dense to allow the internal changes to take place, has yet become, even in the granular substance, too firm for a clean break to be formed between the daughter nuclei.

Judging from these observations on *Zea Mays*, the vexed question whether nucleolar division does or does not always precede nuclear fragmentation, is not one to be answered in a sentence. The age, the general density of the nucleus in that particular tissue or plant, the intensity of the impulse to divide, all have to be carefully considered. The discordant statements made on this subject by eminently careful observers are to be explained by their having examined the same tissues at different ages or grown under different conditions (cf. 6, 11, and 17).

Before leaving the subject it should be mentioned that these older nuclei which are fragmenting by constriction never show karyokinetic figures, and have apparently lost the power of dividing indirectly.

As has been mentioned, the above observations were made on the leaf-sheaths and root-apices of *Zea Mays*. In the latter the directly and indirectly dividing nuclei could be found in adjoining cells, as was the case in the leaf-sheaths. Multinuclear cells of a similar appearance have also been observed in the stem of *Zea*, in the leaf-sheaths of *Secale cereale* (rye), the leaf-sheaths and young stems of *Triticum vulgare* (wheat), the leaf-sheaths of *Hordeum sativum* (barley), and *Dactylis glomerata* (cock's-foot grass).

In conclusion, I should mention that the best results were obtained from young seedlings of the plants mentioned; plants of *Zea*, a little over a foot high, made excellent material.

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EXPLANATION OF PLATES I. AND II.

Figs. 1-8.—Parenchyma cells from leaf-sheaths of *Zea Mays*. Fig. 2 in longitudinal section; the rest transverse.
Figs. 9-13.—Cells from the growing point of the root of *Zea Mays*. Longitudinal section.
Fig. 14.—Parenchyma cells from leaf-sheaths of *Secale cereale*. Transverse section.
Fig. 15.—*a*. Parenchyma cell from leaf-sheath of *Triticum vulgare*.
b. Parenchyma cells from stem of same. Transverse section.

FRESH FACTS.

MEMORY IN FISHES. L. EDINGER. "Haben die Fische ein Gedächtnis?" Das Ergebniss einer Sammelforschung mitgetheilt in der neurologischen Sektion der Versammlung Deutscher Naturforscher und Aerzte in München, 1899. Sonderabdruck aus der Beilage zur *Allgemeinen Zeitung*, Nos. 241 and 242, vom 21 und 25 Oktober 1899. München, 30 pp. We have been favoured with a copy of this interesting paper, embodying the results of observations which have been communicated to Prof. Edinger since he made his appeal for information a couple of years ago. It seems to be clear (1) that the instinctive impulse to flee from certain impressions may be lessened as the fishes find the stimulus harmless and become accustomed to it, and, contrariwise, that they may become shy; (2) that the optic or chemical stimulus normally associated with food may be replaced by the image of the feeder. There is therefore a kind of memory, but it is very different from that of mammals. It may be recalled that Prof. M'Intosh, of St. Andrews, was one of those who answered the question, "Have fishes a memory?" with a decided affirmative, when the matter was discussed a couple of years ago.

MORE TRACES OF NEOMYLODON. ERLAND NORDENSKIÖLD. "Neue Untersuchungen über Neomylodon listai," *Zool. Anzeig.* xxii. 1899, pp. 335-336. The author has made laborious excavations in the cave at Ultima Esperanza (South Patagonia), where previous digging discovered the pieces of skin, etc., belonging to the somewhat shadowy creature, *Neomylodon listai*. He has found a number of bones which he thinks should go with the skin. Their description will be awaited with interest.

CONCERNING AN ANCIENT FISH. A. SMITH WOODWARD. "Note on Scapanorhynchus, a Cretaceous Shark apparently surviving in Japanese Seas," *Ann. Nat. Hist.* iii. 1899, pp. 487-489. A shark in all essential respects identical with the supposed extinct genus *Scapanorhynchus* has been obtained from the deep sea off Yokohama, and described by Profs. D. S. Jordan and Mitsukuri. It has been called *Mitsukurina*, but Mr. Smith Woodward points out that in all the generic characters which can be compared it agrees with the above-named predaceous shark of the Cretaceous Seas.

RÔLE OF THE NUCLEUS. JACQUES LOEB. "Warum ist die Regeneration kernloser Protoplasmastücke unmöglich oder erschwert?" *Arch. Entwickelungsmechanik*, viii. 1899, pp. 689-693. In this paper, which the author has been kind enough to send us, there is a fresh suggestion rather than a fresh fact. The suggestion is that the nucleus is the oxidation-organ of the living substance, and that non-nucleated fragments of cells are incapable of regeneration because the oxidation-function has sunk below the required minimum. The fragments gradually die of asphyxia. This should be compared with the observations of Schenk referred to in another part of this number of *Natural Science*.

A CONTRIBUTION TO EXPERIMENTAL EMBRYOLOGY. JACQUES LOEB. "Ueber den Einfluss von Alkalien und Sauren auf die embryonale Entwickelung und das Wachsthum," *Arch. Entwickelungsmechanik*, viii. 1899, pp. 631-641, 1 pl. Experiments on the developing larvae of the sea-urchin *Arbacia* show that even extremely minute additions of sodium hydrate solution to the sea-water hasten the development and growth, while acids have the reverse effect. The reason suggested is that weak alkalies promote the oxidation-processes and therefore the synthetic processes in the living substance.

A TRIASSIC CUTTLEFISH. K. PICARD. "Ueber Cephalopoden aus dem unteren Muschelkalk bei Sondershausen" (*Zeitschr. deutsch. geol. Ges.* li. pp. 299-309, pl. xvi. Oct. 1899) describes *Campylosepia triasica*, n.g. et sp. on

the evidence of a curved rostrum and an impression of the pro-ostracum ; a few of the septal lamellae are preserved. The author regards it as belonging to the Sepiadae and as a link between the belemnites and cuttlefish, and he compares it with *Belosepia*. The latter, however, is a Tertiary form, and no true *Sepia* is known from Mesozoic rocks. This fact, while increasing the interest of the discovery, leads us to ask for more evidence.

AUTOGAMY IN PRIMULACEAE. A Field Naturalist, M.A. Camb. "The Primrose and Darwinism," *London Quarterly Review*, clxxxiv. October 1899, pp. 209-235. This very interesting and circumstantial indictment of Darwin's conclusions in regard to cross-fertilisation in primroses occurs in a place where it may be overlooked by many botanists. After relating his observations and stating his criticisms, the unknown author says: "It is not possible from the above considerations in reference to the method of Darwin's experiments, and especially also from the above case of the primrose, to avoid the conclusion that Darwin has not established his theory that cross-fertilisation is necessary to the full fertility of flowers. On the contrary, we are of opinion that the primrose gives strong confirmatory evidence to Axell's view, that under *natural* and equal conditions self-fertilisation of flowers is both the *legitimate* fertilisation and the most productive."

ELIMINATION IN SPARROWS. HERMON C. BUMPUS. "The elimination of the unfit as illustrated by the introduced sparrow, *Passer domesticus*," a fourth contribution to the study of variation. Eleventh Lecture in Biol. Lectures at Wood's Holl in 1898. Boston, 1899, pp. 209-226. After a severe storm a number of English sparrows were brought to the anatomical laboratory of Brown University. Seventy-two revived ; sixty-four perished ; and the author has made a careful comparison of the eliminated and the surviving. He has reached three conclusions :—(1) That the birds which perished were eliminated because of deficiency in certain structural characters possessed by the survivors ; (2) the process of selective elimination is most severe with extremely variable individuals, no matter in what direction the variations may occur ; (3) disregard of structural qualifications finally produces a throng of degenerates, whose destruction will follow the arrival of adversity.

ANOTHER ENIGMA. RICHARD HEYMONS. "Ueber bläschenförmige Organe bei den Gespenstheuschrecken. Ein Beitrag zur Kenntniss der Eingeweidenervensystems bei den Insecten," *Sitzber. Preuss. Akad. Berlin*, 1899, pp. 563-575, 2 figs. In the head of a European stick-insect, *Bacillus rossii*, there lie near the gullet, and associated with the pharyngeal ganglion, two little vesicles of ectodermic origin which are very puzzling. They are neither ganglionic nor glandular, and contain a central chitinous spherule surrounded by several concentric chitinous lamellae. Perhaps they are comparable to the "corpora allata" which occur in a number of other insects, but they are not the same in detail, and besides we do not know what the "corpora allata" are. Heymons tried by experiment to find out something about their function, but the result was inconclusive. He leaves their nature an enigma, except that he suggests that they may have something to do with the visceral nervous system.

NEW PELAGIC NEMERTEAN. W. MCM. WOODWORTH. "Preliminary account of Planktonemertes agassizii, a new pelagic Nemertean," *Bull. Mus. Comp. Zool. Harvard*, xxxv. 1899, pp. 1-4, 1 pl. This new form, like the only other known genus, the "Challenger" *Pelagonemertes*, was taken in the Pacific Ocean from considerable depths. In its leaf-like body, hyaline structure, rhynchocoelom as long as the body, unarmed proboscis, dendrocoelous gut, and absence of cephalic grooves or organs of special sense, it resembles *Pelagonemertes ;* but its distinctive features are : a common external opening for mouth and proboscis, supraoesophageal ganglia smaller than the suboesophageal, the presence of a median dorsal vessel, and the large number of lateral diverticula of the intestine.

SOME NEW BOOKS.

THE SPRINGS OF CONDUCT.

The Origin and Growth of the Moral Instinct. By ALEXANDER SUTHERLAND, M.A. In two vols. Pp. xiii. + 461, vi. + 336. London: Longmans, Green, and Co. 1898.

We need make no apology for reviewing this interesting work in *Natural Science*, for, as the author tells us, full half of the book is a detailed expansion of the fourth and fifth chapters of the "Descent of Man." "Darwin showed in these chapters a noble gift of insight, but to have made good his position from point to point, to have left nothing behind him unreduced, would have demanded a labour which neither his own health nor the length of an ordinary life would have permitted." Mr. Sutherland has done good service in filling in Darwin's scheme. Many persons have made *vorläufige Mittheilungen* on the same subject, but Mr. Sutherland has written a treatise of great value. If he had submitted his two volumes to a candid friend at a distance, who rejoiced in the exercise of the blue pencil, if he had made the two volumes into one, if he had avoided such question-begging phrases as "the moral instinct," if he had called his book "The Evolution of Sympathy," he would have commanded an interested audience whom this treatise will never touch. We would not seem ungrateful, the book is the outcome of eleven years of hard work, it is full of careful erudition, it is most intelligibly written; our regret is simply that a lack of worldly wisdom or self-criticism has robbed the book of much of its utility by leaving it so large.

Nowhere else that we know of can we find such a carefully selected treasury of facts bearing on parental care, conjugal affection, and the feeling of kinship—all working towards a theory of the evolution of sympathy on which the author believes morality to be founded. He shows, to our thinking conclusively, how there was worked out among animals an inheritance of altruistic emotions which became in man the springs of good conduct; but we do not think that he has been equally successful in showing how man became moral, that is, became accustomed to "think the ought," to control his conduct in reference to general ideas and ideals. According to Mr. Sutherland, "the moral instinct is in social animals the result of that selective process among the emotions which tends to encourage those that are mutually helpful, and to weaken those that are mutually harmful," but what he has actually been working at is the growth of sympathetic emotions, not the origin of the distinctively ethical note which characterises many human actions. The evolution of altruistic feelings is one thing; the distinction between good behaviour and moral conduct is another; and the book seems to us to fail seriously in not appreciating the distinction. But as a treatise on the evolution of sympathy, on the springs of good conduct, it is admirable, and most useful to biologist and moralist alike.

X.

THE TIDES.

The Tides Simply Explained, with Practical Hints to Mariners. By the Rev. J. H. S. Moxly, B.A., T.C.D. Pp. v. + 151. Rivingtons: London, 1899.

There are many excellent points about this little book, although we are not prepared to accept all the positions taken up by the author. Laplace, Airy, Kelvin, and G. H. Darwin, our recognised exponents of tidal theory, are severely dealt with, especially the last named. The author's object is to show that the much-reviled "Equilibrium Theory"—the theory, in fact, which is usually described in our elementary physical geographies—is, in a slightly modified form, amply sufficient to explain all tidal mysteries. Consequently he argues strongly against the rival kinetic or perturbation theory as it might be termed. In his criticism of Darwin's dictum that a vertical force cannot produce sideways motion, he seems, however, to confuse vertical force with vertical pressure; and there is a good deal of loose and inaccurate reasoning between pp. 38 and 40—reasoning which fortunately does not affect the main design of the book. In the constructive part of his book Mr. Moxly is decidedly at his best, and we are not acquainted with any clearer statement of the results that (with the assumption of the two tides each day) naturally follow from the equilibrium theory. In some of his discussions he is particularly happy, as, for example, in his account of the single daily tide in high latitudes, of the high tides in the Bay of Fundy, and other anomalies. C. G. K.

A TREATISE ON CRYSTALLOGRAPHY.

Crystallography. By W. J. Lewis, M.A., Professor of Mineralogy in the University of Cambridge. Cambridge Natural Science Manuals, Geological Series. Pp. xii. + 612, 553 figs. The University Press, 1899. Price 14s. net.

This substantial volume is a very full and complete geometrical treatise, covering much the same ground as Professor Maskelyne's "Morphology of Crystals," which was published by the Oxford University Press in 1895. The aim of the author is evidently to supply the practical needs of university students, to whom the drawing and calculation of crystals measured in the laboratory is an exercise by which a knowledge of Crystallography can best be obtained; two chapters of considerable length devoted to the subject of crystal drawings and projections are accordingly introduced at an early stage, and the general description of the various classes of crystals, which occupies the greater portion of the book, is illustrated by abundant examples of drawings and calculations very fully worked out. Frequent references to crystals in the University Collection indicate that the book contains many original observations.

The chapters on the systems and their various subdivisions are followed by a chapter on twin crystals, which occupies about 100 pages, and is probably the most extensive general treatment of this subject which has been published since Sadebeck's volume on "Applied Crystallography."

The book presents the appearance of a mathematical treatise, and may discourage the non-mathematical student, but the methods of proof employed will be found to be in reality simple and so expressed as to demand no advanced mathematical knowledge; they are consequently somewhat laboured and lengthy, but the more concise and elegant treatment by analytical methods is given in a chapter at the end of the volume.

A short chapter on the physical properties of crystals is introduced merely

for the purpose of justifying the classification into seven systems; frequent reference to the optical characters is also made in the detailed description of various crystals.

Several new terms are employed; most of the classes formerly known as hemimorphic are called acleistous; monoclinic crystals are divided into a gonoid, a plinthoid, and a hemimorphic class; an axis of symmetry which is polar is called uniterminal, a name that appears awkward where all the other terms are of Greek origin.

The representation of crystal axes by interrupted dots and dashes is of doubtful expediency, since they become difficult to distinguish among other lines; but the author ingeniously makes use of this contrivance to indicate their "order" when they are axes of symmetry by the numbers of dots. The number of the chapter might have been given at the head of each page, for frequent references are made to previous chapters.

The reader will perhaps not expect to find a philosophic treatment of the principles which underlie the geometry of crystals in a book designed for the practical instruction of students; if the subject be new to him he may wonder why a crystal is treated merely as an isolated problem in drawing, projection, and calculation; but let him master the contents and we think that he will acquire a very considerable knowledge of geometrical crystallography, which will set him thinking about the signification of the beautiful laws which prevail in this subject.

CLASSES OF CRYSTALS.

Darstellung der 32 möglichen Krystallklassen. By H. BAUMHAUER. Leipzig: Engelmann, 1899; 36 pp., 32 figures, and 1 plate.

Much attention has recently been paid to the subject of crystal symmetry, and the treatment and nomenclature of the 32 classes have undergone many changes.

In this short pamphlet Professor Baumhauer adds another to several attempts which have been made to describe these classes in a simple and systematic manner. It will be sufficient to say here that he classifies them according to their axes of symmetry, and distinguishes between those which are and those which are not intersections of symmetry planes by the not very happy terms homogeneous and inhomogeneous; axes perpendicular to a symmetry plane are distinguished by the equally unsatisfactory term symmetrical.

The plate which gives a summary of the classes succeeds perhaps better than any previously published, in making the symmetry apparent, and will be of use to teachers.

ROCK-ANALYSIS.

Praktische Anleitung zur Analyse der Silicatgesteine. Pp. 86. Leipzig: W. Engelmann, 1899.

This, as its title further sets forth, is a translation by Dr. E. Zschimmer of the introductory portion of Bulletin No. 148 of the United States Geological Survey, the concluding part of that work being a most valuable series of rock analyses. These analyses are omitted in the translation, which deals only with the analytical methods adopted by Prof. F. W. Clarke and Dr. W. F. Hillebrand in the laboratory of the United States Geological Survey. The methods aim at an exactitude in rock analyses hitherto found wanting in the published work of many of the earlier chemists who have contributed to our knowledge of the composition of rocks. In the analyses given in the Bulletin just cited, extremely small quantities of elements have been detected and estimated in

rocks in which their presence would hardly have been suspected, and which in ordinary analyses would have been unsought and overlooked. The translation contains some additional remarks upon and references to the later observations of Dr. Hillebrand, and on p. 33, a woodcut, not given in the original Bulletin, illustrates the construction of a modified form of Gooch's apparatus, as employed in the U.S. Survey laboratory for the determination of combined water. The translation is well printed and has an index, but although it is a most useful and convenient publication, the original Bulletin No. 148, with its well-tabulated analyses, will probably be more frequently consulted in this country than Dr. Zschimmer's careful translation.

MESOZOA AND SPONGES.

Traité de Zoologie Concrète. Leçons professées à la Sorbonne. Tome ii. 1re Partie. Mésozoaires, Spongiaires. By YVES DELAGE and EDGARD HÉROUARD. Pp. ix. + 244, with 15 coloured pls. and 274 figures in the text. Paris: Librairie C. Reinwald, Schleicher Frères, 1899. Price 12s. 6d.

The new instalment of this great work sustains the high level of its predecessors in its fulness and clearness of exposition, and in its liberality of excellent illustrations. The first part contains the fullest connected account as yet published by the so-called Mesozoa, and is therefore of great interest. Four classes are recognised:—(1) Mesocoelia, viz. *Salinella;* (2) Mesenchymia, including *Treptoplax* and *Trichoplax;* (3) Mesogonia, comprising Dicyemiae and Orthonectiae; and (4) Mesogastria, viz. *Pemmatodiscus.* An appendix treats of *Physemaria, Cementaria, Pompholyxia, Kunstleria,* and *Siedleckia.* The authors have conferred a great boon on zoology, in bringing together the available information in regard to these obscure creatures which are as interesting as they are puzzling.

The second part deals with the sponges, to our knowledge of which Prof. Delage has made some notable contributions. As was expected, there is a careful discussion of the affinities of the class, in which Delage's own views are naturally followed, though the diversity of opinion is duly recognised. The classification adopted is as follows:—I. Calcaria, including Homocoelida and Heterocoelida; II. Incalcaria, including Triaxonia (Hexactinellida and Hexaceratida) and Demospongiae (Tetractinellida, Monaxonida, and Monoceratida). An appendix deals with the doubtful Abyssospongea, which probably do not deserve the name. A zoological treatise on different lines may well be conceived, but it will be hard to excel this one in clearness and fulness, or in beauty of illustration.

ELEMENTS OF VERTEBRATE EMBRYOLOGY.

Die Elemente der Entwickelungslehre des Menschen und der Wirbelthiere. Anleitung und Repetitorium für Studierende und Aerzte. By Dr. OSCAR HERTWIG, Director of the Anatomical-Biological Institute of the University of Berlin. 8vo, pp. vi. + 406, with 332 figs. Jena: Gustav Fischer, 1899 [dated 1900]. Price 7.50 marks, 8.50 bound.

In twelve years Prof. Hertwig's well-known *Lehrbuch* has passed through six editions, and has been translated into English, French, Italian, and Russian; and no one who has used it, whether as student or teacher, will wonder at its great success. It is a model of lucidity, it is well illustrated, it is flavoured with the salt of general ideas, and it is full of suggestion.

But as he worked at the later editions, Prof. Hertwig began to feel that it was impossible to cater for two sets of appetite. The expert wished for more

detail and the student for less. Hence the publication of the present "Elements" which is adapted to the busy student, while further editions of the *Lehrbuch* will be specialised for his teachers. Beyond that some paragraphs have incorporated recent results, there is little to distinguish this new volume from its predecessors, but it is shorter and perhaps simpler, and more emphasis is given to the summaries. All must wish it good speed.

CO-OPERATIVE GEOGRAPHY.

The International Geography. By SEVENTY AUTHORS. Edited by HUGH ROBERT MILL, D.Sc., etc. Pp. xix. + 1088. London: George Newnes, Limited, 1899. Price 16s.

The method of compiling a hand-book of geography by the collaboration of a number of authors, each of whom is a native of the country he describes, or has had especial opportunities of making himself thoroughly conversant with the subject of which he treats, has self-evident advantages. It has, however, its drawbacks. Authors are apt to give undue prominence to their particular theories, and to entertain different views as to what details should be included, with the consequence that the book is lacking in uniformity. For the contributors to this work the editor has drawn up a set of rules, setting forth the heads of information and the order in which they should be discussed. But these rules have not always been followed. In some cases the geology has not been touched upon, in others internal communications have received little attention, and boundaries are sometimes clearly defined where they are definitely fixed and marked on maps, while no mention is made of others which are still subjects of dispute.

Two or three instances will suffice to show how the division of labour has resulted in a want of unity in the whole. On p. 16 it is stated that Thales invented gnomonic projection, and the reader will naturally turn—in vain—to the preceding chapter to find out what that projection is. The writer of the chapter on Mathematical Geography has evidently had some difficulty in compressing all he had to say into the space allotted, and could not foresee that his colleague would mention a projection now used only on charts for Great Circle sailing. The apparent antagonism contained in the sentences, "Further north the Parana takes the name of Paraguay" (p. 850), and "They (the Parana and Paraguay) both rise in Brazil," needs only a few words of explanation. Another case is of more importance, the two sentences being contradictory; on p. 423 we are told that "no definite geomorphic line divides them (the islands of the archipelago between Asia and Australia) into an Asiatic and an Australian group. 'Wallace's Line' . . . is only a faunal boundary"; and on p. 533 we find, "This line, therefore, clearly follows what, in very recent geological times, was the shore of the continent of Asia"—a boundary still marked by a belt of deep water.

Other small discrepancies might be pointed out, and are to be expected in so comprehensive a work on its first appearance, and considering the vast amount of labour involved in gathering together so large a staff of collaborators, providing for the translation of articles written in foreign languages, and in the general supervision of the whole work.

Nevertheless, a large measure of success has been attained, the individual chapters are on the whole of a high order, many of them being really excellent, more particularly those from the pens of professed geographers, who are accustomed to regard a country from all points of view, and treat the physical features, geology and climate, in connection with the occupations of its inhabitants. Professor W. M. Davis' description of the United States deserves especial mention, and the editor's chapter on the British Isles has also great merit, apart from one or two slips in the historical paragraphs.

When a new edition is required, as no doubt it will be, some alterations should be made in the allotment of space, on the basis of the importance of the subject, and not on a comparison of text-books; for "The International Geography" is more than a mere text-book. It may be found advisable to slightly extend the work, so as to form two volumes of more moderate size. Space might also be profitably saved by the omission of the historical paragraphs. A history of a country compressed into a single paragraph is useless, nor has history a *locus standi* in a geographical work unless it be connected with the physical features. Except Professor Davis, who indicates briefly how the natural features determined the lines of penetration into the American continent, hardly any author gives more than a few bare historical facts.

In conclusion, we must give a word of praise to the numerous small sketch-maps and diagrams scattered throughout the volume. They are clearly and carefully drawn, and exhibit the particular facts or phenomena they are designed to impress on the reader, unobscured by unnecessary details.

PALAEONTOLOGY IN MINIATURE.

Paläontologie. By Dr. RUDOLF HOERNES, Professor in the University of Graz. Pp. 212, with 87 figs. Leipzig: G. J. Göschen, 1899. Price 80 pf.

This primer of palaeontology is a little marvel. Of a size familiarised to many students of ten years ago by Macalister's "Zoology" and MacNab's "Botany," with excellent type, with over fourscore excellent figures, and with over 200 pages of sound, descriptive palaeontology by a well-known authority, it costs about nine pence! We should like to hear the comments of a British publisher on this the 95th number of the Sammlung Göschen. After an introduction of 32 pages on the scope and aims of palaeontology, there are 50 pages on the plants of the past, and more than 100 on the animals. The author has been more conservative than was necessary, thus Spongiae are ranked under Coelenterata, Anthozoa precede Hydrozoa, Vermes are separated by Echinodermata from Bryozoa and Brachiopods, and so on. As we peruse it, however, some of the simplicity of the table of contents disappears, for while Pisces form the first class of Vertebrata, it is expressly noted that forms like *Amphioxus* and *Palaeospondylus* may well be referred to special classes. We have great admiration for this little book, but would make two general criticisms:—(1) that there is too little suggestion of history, of movement, of progress, of evolution, in short, of the keynote of a true palaeontology; and (2) that in a primer, above all, insecure conclusions should be very cautiously stated, and surely the doctrine of Kinetogenesis, for instance, which finds a prominent place in the introduction, is still an insecure generalisation.

Missouri Botanical Garden Tenth Annual Report. St. Louis, Mo., U.S.A., published by the Board of Trustees, 1899.

This well-known publication sustains its former high standard in every respect. The present report is specially valuable, because it gives a résumé of the work of the Garden for the first decade of its existence, 1889-98; and also a complete index, of 51 pages, of the ten volumes of the Record.

The objects of the late Mr. Shaw, the founder, are summarised, and not the least important of these is the encouragement of botanical research in the broadest sense. The endowment of the generous founder enables these objects, which he contemplated, to be carried out successfully, and permits of the gradual growth and extension of the Garden in all its branches.

The interchange of seeds, cuttings, and small plants with similar institutions

abroad, is another noteworthy feature of the Garden. The herbarium grows apace, and plans have been prepared for the construction of a museum. The library alone ought to attract students from a wide area; it has 32,000 books, and over 200,000 index cards; but it is specially rich in pre-Linnean works, over 500 of which were gifted in 1892 by the late Dr. Ed. Lewis Sturtevant.

An able biographical sketch of Dr. Sturtevant by Prof. C. S. Plumb is given in the Record. Sturtevant did much for the cause of agriculture in America, and his name will long be associated with the famous herd of Ayrshire cattle which he established, and with the cultivation of maize.

A short illustrated paper on "A Sclerotoid Disease of Beech Roots," by Hermann von Schenk, appears as research work from the Garden. The condition was found on one clump of trees, and their roots were devoid of the fungus-covering or mycorhiza common to beeches and most other forest trees. Further investigation may throw light on this unusual condition.

The most important part of the Report consists of a paper by F. Lamson-Scribner on "Notes on the Grasses in the Bernhardi Herbarium, collected by Thaddeus Haenke, and described by J. S. Presl." This article consists of 25 pages of letterpress and 54 full-page plates, which will be admired by all students of the Gramineae; the drawings are by Mrs. M. D. B. Willis, *née* Baker, and they recall to British botanists the faithful work of Parnell, but in the matter of reproduction, on paper of superior finish, the former surpass the latter. The collection includes a large number of American species from the Pacific coasts of Mexico, California, and Peru; many from the Philippines, and a few from Nootka Sound. Most of the genera, and certainly the facies of these grasses, resemble those of South Africa. One word of caution may be permitted; the terminology of Presl has been adhered to, but the majority of the genera have been revised since 1830, and if the Herbarium of the Missouri Botanical Garden is to be of use to modern students, its classification must also be modern. R. T.

The North American Slime-Moulds, being a list of all Species of Myxomycetes hitherto described from North America, including Central America. By T. H. MACBRIDE, A.M., Ph.D., Professor of Botany in the State University of Iowa. Pp. 231 with 18 plates. New York and London: The Macmillan Co., 1899. Price 10s. net.

This book is likely to form the classic on the subject for North America. Apart from the scientific interest of the work one is compelled to admire the binding, the paper, the type, and the beautiful photo-reproductions of the plates.

The only general works published within recent years on the subject are Massee's "Myxogastres," in 1893; Lister's "Mycetozoa," in 1895: a worthy third is Professor Macbride's "Slime-Moulds," if we may follow the author in giving them this name, for it is unfortunate that botanists cannot agree as to the proper name that should be applied to those organisms which seem to occupy the border line between the two organic kingdoms.

The volume begins with a general description of the vegetation and reproduction of the slime-moulds. It is pointed out that they are not unicellular organisms, as was formerly taught, but multinuclear and karyokinetic. From the resemblance of the protoplasmic mass to that of a giant amoeba arises the claim of the zoologist to consider the slime-mould his special property.

But the author prefers to leave the question of their higher relations alone, recognising that no one test can be applied as a universal touchstone to separate plants from animals. As a matter of fact the study of the slime-moulds rests chiefly with the botanists, and it is expedient to leave it in their hands.

Over 400 species of slime-moulds have been described, and half of these

have been recognised in the United States. One degenerate species, *Plasmodiophora brassicae*, occasions the disease known as "club-root" in cabbage, and "finger-and-toe" in turnip; while it is alleged that *Plasmodium malariae* may be the cause of malarial fever, and if it turn out to be a slime-mould, then the group suddenly acquires an unusual human interest. Apart from these two the slime-moulds are of no economic importance. Advice is given regarding the collecting and preserving of material, but the greater part of the book is taken up with classification. The descriptions of the genera and species are most carefully given, while the measurements of the spores are in microns.

For the sake of the British farmer we wish that the following statement made by Professor Macbride regarding *Plasmodiophora* were true for the British Isles:—

"Careful search continued through several years has not availed to bring this species to my personal acquaintance." It is unfortunately too true, however, that British farmers lose thousands of pounds annually through the ravages of *Plasmodiophora* in their turnip crops. R. T.

A CRITICISM OF THE BIOLOGICAL GOSPEL.

From Comte to Benjamin Kidd. The Appeal to Biology or Evolution for Human Guidance. By ROBERT MACKINTOSH, M.A., B.D., D.D., Professor at Lancashire Independent College. xxii. + 287 pp. London: Macmillan and Co., 1899.

"The appeal to biology, outlined by Comte, newly defined and emphasised by Darwinism, has now been stated in the most extreme form logically possible," by Mr. Benjamin Kidd. Dr. Mackintosh has weighed the results of this appeal in the balances and finds them very short weight. In fact, he indicates that the appeal is gratuitous. There is available elsewhere much better guidance for human conduct than biology can offer, and the appeal to biology is apt to be misleading, as well as unsatisfactory. These are hard words, but it must be remembered that biology is still very young, much too young to give advice. Some have tried to force its hand and the results do not look well, but it was not a fair game to play. The science is too young to become a basis for the art of life.

The author is brilliantly clever; there is not a dull page in the book, perhaps not a dull sentence; his criticisms of even purely biological matters make one feel what the science has lost in his being outside of it. To contradict him is impossible, for he is so reasonable; to correct him is impossible, for the time is not yet ripe; to believe him is (for a biologist) impossible, for he proves too much. It seems to us that biology, preoccupied with its own concrete problems, has simply stammered like a child when forced to confront the big problem of human life; it has something to say, but it is not ready to say it. That it will eventually have a rational word to say, and one which will rhyme with the best word of the moralist, we never doubt.

Part I. deals with Comtism, the appeal to biology, the appeal to history, and the doctrine of altruism. Part II. discusses the "simple evolutionism" of Spencer and Leslie Stephen. Part III. deals with Darwinism, or Struggle for Existence, and includes a splendid chapter on the metaphysics of natural selection. Part IV. has to do with Weismann and Benjamin Kidd, so widely apart, and yet in one respect so near akin. Finally we have a summary and conclusions. It is easy to write these lines; but to criticise is another matter, and we frankly confess that we must refrain, though the temptation is great. We refrain for this reason, that although we are unable to agree with the author's central conclusions, we feel that he has done great service in showing that the appeal to biology is premature. X.

OUR PLAY.

Die Spiele der Menschen. By KARL GROOS, Professor of Philosophy in Basel. Pp. 538. Jena: Gustav Fischer, 1899. Price 10 marks.

Two or three years ago Professor Groos rather startled us by his book "Die Spiele der Thiere," in which he showed that play was one of the most serious things in the world. This book was translated last year by Mrs. Baldwin, and published, with a preface and an appendix by Professor J. Mark Baldwin, under the title "The Play of Animals: a Study of Animal Life and Instinct." The author's thesis has thus become familiar. Play is not mere by-play, but a matter of serious moment; it is the expression of an instinct developed by natural selection, and justified (1) because the playful young animal can rehearse without responsibilities, and practise for its future life without serious consequences, play being really the young form of work; and (2) because the young animal is able in play to learn many lessons which would otherwise have to be inherited as special instincts, thus lessening the burden of inheritance, and putting a premium on intelligence. To which may be added that the play-period affords elbow-room for new departures—an "*Abänderungsspielraum*"—before natural selection begins to operate with its usual sternness.

In the volume now before us Professor Groos applies his "practice theory" of play to the games of children and men, and on the whole seems to succeed in corroborating it, though the case does not seem to us quite so clear as it was when animals alone were dealt with. The first section deals with playful experimenting—sensory, motor, intellectual, and emotional. The second section discusses combative play, love play, imitative play, and social play. Then follows a general consideration of the theory of play, looked at from six points of view—physiological, biological, psychological, aesthetic, sociological, and educational.

We do not know whether to admire most the author's erudition, or his vivacity, or his intellectual perspective. The result is certainly a notable contribution to the theory and art of life. It invests the familiar adage, "All work and no play makes Jack a dull boy," with a profoundness of solemn meaning.

It is to be hoped that this volume will also be translated by Mrs. Baldwin, who dealt so successfully with the first, for it is a book that ought to have the widest possible circulation, not merely because it is a thorough vindication of what we may call the Darwinian theory of play, but also for its practical suggestiveness to parent and teacher, physician and artist.

MATSCHIE'S CATALOGUE OF FRUIT-BATS.

Die Fledermäuse der Berliner Museums für Naturkunde: 1 Lieferung, Die Megachiroptera. By P. MATSCHIE. 8vo, pp. viii. + 103, pls. 14. Berlin: George Reimer, 1899. Price 24 marks.

The British Museum "Catalogue of Chiroptera," by the late Dr. Dobson, having been published so far back as 1878, has long been completely out of date; and naturalists should therefore welcome Dr. Matschie's new descriptive synopsis, of which the first instalment is before us. It appears that the late Professor Carl Peters, Director of the Berlin Museum from 1857 to 1883, contemplated the publication of a monograph of the Bats, for which were prepared no less than 75 lithographic plates, executed by the well-known artists F. Wagner and G. Mützel. These plates remained in the hands of Herr G. Reimer, the publisher, after the death of Professor Peters, but no accompanying MS. was found among the effects of the latter. This being so, Dr. Matschie determined to write the text for a descriptive synopsis of the

order *de novo*, while the publisher undertook to supply such additional plates as were required to bring the work thoroughly up to date. At least 15 of such new plates are announced for issue, 11 of which have been drawn and lithographed by the late Mrs. Matschie. With such a wealth of illustration, the work starts with a strong promise of success. It is announced to be completed in four parts.

The present fasciculus deals with the important and interesting group of Megachiroptera or Fruit-Bats, all the members of which Dr. Matschie follows his predecessors in placing in the single family *Pteropodidae*. In this family the author recognises 20 genera and 122 species, together with numerous sub-genera and sub-species. And here it may be remembered that, although the distinction is clear enough in the systematic index, it would have been better if the number of sub-species had been more markedly distinguished in the text from those of species. Moreover, to our thinking, a few more plates of the animals themselves, in addition to the numerous figures of skulls, would have added decidedly to the general interest of the fasciculus, and have made it more attractive at least to the amateur naturalist.

In regard to the limits of genera the author differs considerably from some English naturalists. He regards, for instance, the curious *Pteralopex atrata*, of the Solomon Islands, as representing merely a sub-genus of *Pteropus*, instead of a genus by itself; while, on the other hand, *Cynopterus marginatus* from Sarawak, described by Mr. O. Thomas in 1893, is considered worthy of separation as a distinct genus (*Dalionycteris*). Moreover, there are several important emendations on the Dobsonian nomenclature, *Rousettus*, Gray, replacing *Xantharpyia*, Gray, while *Gelasinus*, Temminck, stands for the preoccupied *Harpyia*, Illiger. If this latter change can be substantiated it will save the transference of the name *Cephalotes* from the genus it usually stands for to the above-named group (*Harpyia*), as has been proposed by Mr. T. S. Palmer; but it is very doubtful whether experts will admit the innovation. Although changing preoccupied names when they are literally identical with their precursors, Dr. Matschie refuses to admit that a name like *Macroglossa* necessitates the abolition of *Macroglossus*; but here, again, we are on dangerously debatable ground.

So far as we have tested them, the generic and systematic definitions seem clearly and accurately drawn up; but how these work in actual practice can only be demonstrated when new genera or species have to be described. Special value attaches to the author's notes on the distribution of the species of Epauletted Bats (*Epomophorus*) in Africa, and the zoo-geographical sub-regions of that continent, but there seems too much tendency to make the species fit in with the regions.

MULTIPLICATION OF MOSSES.

Untersuchungen uber die Vermehrung der Laubmoose durch Brutorgane und Stecklinge. Von Dr. Carl Correns. 8vo. Pp. xxiv. + 472, with 187 figures. Jena: G. Fischer, 1899. Price 15 marks.

By no means the least exciting group of plants are the Mosses. How interesting their position in the scale of plant-life, so far removed, excepting only their close allies the Liverworts, from everything else, and separated by as great a gulf from the less highly organised Algae as from the more highly organised Ferns! How remarkable their life-history, with its clearly marked division into two phases, distinct but never separated! How puzzling the comparison of organs and members with those of the higher plants! On the one hand the leaf that is not a leaf, on the other hand the unmistakable leaf-like character in structure and function of the base of the highly-organised spore-capsule. But perhaps the most striking feature of the group is their

power of reproducing vegetatively. Almost any portion of the plant will, under favourable conditions of moisture and temperature, give rise to a new individual, and there are also a great variety of means by which this can be naturally effected. It is this last aspect of their biology which forms the subject of Dr. Correns' substantial contribution to Muscology, a work which we are sure will be perused with much interest by the increasing number of botanists who are specially interested in the Mosses. The book, which is partly special, partly general, opens with a short introduction (pp. xvii.–xxiv.), in which terms are explained and methods described. The "brood-organs" are in brief those organs which are definitely produced for the purpose of vegetative reproduction; the "Stecklinge," on the other hand, are those parts of the plant which will on separation form a new individual, but have not been definitely developed to that end. The greater part of the book (pp. 1–322) is "special," comprising first an account of the investigated cases of multiplication by "brood-organs," arranged systematically in tribes, families and genera, and secondly those species in which the other method obtains. This is followed by a "general" part (pp. 325–360), arranged in five sections, treating of the morphology and phylogeny, structure, development and germination, more especially of the "brood-organs," and of their value for systematic purposes. A bibliography occupies a few pages, and the book closes with an index of the plants mentioned in the text. One cannot have too many illustrations in a work of this kind, and the 187 which are distributed through the text form a valuable help to the elucidation of the subject-matter. R.

ANOTHER BOOK ON BACTERIA.

Bacteria, especially as they are related to the Economy of Nature, to Industrial Processes, and to the Public Health. By GEORGE NEWMAN, M.D., F.R.S.E., D.P.H. (Camb.), etc., Demonstrator of Bacteriology to King's College, London. Pp. viii. + 351, with 15 micro-photographs by E. G. Spitta. London: John Murray, 1899. Price 6s.

In his short preface the author expressly disclaims any attempt to write either a record of original work or a laboratory text-book. His object is merely to discuss in a popular scientific form the present state of knowledge concerning bacteria. As the title indicates, the bacteria considered are not only those capable of producing pathogenic effects, but include the vast number of those which are concerned in natural and industrial processes.

Embracing so wide a range of subject, and being designedly written to suit the lay reader, the descriptions are unavoidably often sketchy and incomplete. But the author is in his happiest vein when dealing with the rôle of bacteria in natural processes, and with their industrial application, and to this the greater part of the book is devoted. The chapters on the bacteria in the soil, the bacteria of fermentation, and the bacteria of milk and its products, are specially valuable to medical readers for the lucid and interesting account they give of the far-reaching beneficial effects of bacteria. The ordinary student of medicine is too apt to associate bacteria with disease alone, and the author is to be congratulated on presenting in so attractive a form an outline of the immensely greater activities which these lower vegetable organisms possess. Whether the brief description of the chief pathogenic bacteria could be of equal value or interest to the lay reader, we are inclined to doubt, and it would be easy to criticise adversely some of the details given in this section. But the shortcomings of this latter part are only of minor importance, and do not detract from the value of the preceding chapters.

The book is illustrated by several good illustrations from micro-photographs, and by a number of outline drawings of bacteria, for which the writer claims only a diagrammatic significance. It may be permitted to point out that some

of the latter have scarcely even that claim—notably the drawing of the bacillus of malignant oedema on page 174.

We would, however, cordially recommend the book to all who desire to gain an introduction to the vast science of bacteriology, and, more particularly, to medical men who take any interest in natural processes outside, but intimately related to, their immediate profession. D. A. WELSH.

PRACTICAL CHEMISTRY.

Laboratory Manual—Experiments to illustrate the Elementary Principles of Chemistry. By H. W. HILLYER, Ph.D. New York: The Macmillan Company; London: Macmillan and Company. Pp. vi. + 200 (100 pages blank). Price 4s. net.

The character of this book fully corresponds to its second title, and the teacher of elementary students both in school and college will find it useful in laboratory work. The book is divided into two parts, Part I. dealing with preparation and properties of the elements and their compounds, whilst Part II. is a guide to experiments in verification of quantitative laws. The experiments are, on the whole, very well chosen, and the directions for their performance are definite and accurate, illustrative diagrams being given where necessary. In the last section, dealing with "Molecular weight by chemical means," scarcely sufficient stress is laid on the fact that the basicity of the acids to which the method is applicable must be determined beyond all doubt if a conclusive result is to obtained. Students almost invariably ignore this essential condition, so that the point ought to be specially emphasised.

A FRENCH CONCHOLOGY.

1. Les coquilles marines des côtes de France. By M. Locard. Large 8vo, pp. 384, with 348 figures in the text. Paris: J. B. Baillière et Fils, 1892. Price 18 francs.
2. Les coquilles marines au large des côtes de France. By M. Locard. Large 8vo, pp. 198. Paris: J. B. Baillière, 1899. Price 6 francs.

By the issue of the second of the volumes above mentioned, Mons. Locard has completed the publication of his "Conchyliologie Française." As stated in the introduction to the first volume, his object was to give short but precise descriptions of all the species of shells which are to be found in French waters, so that the student and collector might be able to name his specimens without having recourse to large and expensive works.

The first volume, published in 1892, dealt with the shells of the French coasts. A second, published in 1893, gave descriptions of those living in the fresh and brackish waters of the country. A third, published in 1894, described the terrestrial shells; and now the whole is completed by a volume on the marine shells found outside the French coasts between the coralline zone and a depth of about 2000 metres.

We propose to notice the first and last of these volumes, which contain descriptions of all the genera and species of shells that have been found in the seas around France. Mons. Locard's work is essentially a conchological one. The animals themselves are not described; neither are questions of classification or synonomy touched upon, the reader being referred for these points to his previous work; the "Prodrome de Malacologie Française." But he defines the families and genera which he has adopted, and gives a description of each species of shell, with a mention of its geographical and bathymetrical distribution. Many of the species are illustrated by figures in the text, of which there

are 348 in the first volume, but there are no illustrations in that on deep-water shells.

Most of the figures are either of natural size or are enlargements of small species, but those of larger shells are unequally reduced, some being one-third, some one-half, some two-thirds of actual size, while one (*Tritonium nodiferum*) is only one-eighth of such size, which gives a false impression of its dimensions. The figures are not woodcuts, but are photo-prints from good drawings, and suffice for purposes of recognition.

In the first volume he enumerates 1186 species, including 14 Brachiopods, but there are many among them which other conchologists would probably regard as varieties. The numbers of each class are as follow :—

Gastropoda	777
Scaphopoda	11
Lamellibranchiata . . .	384
Brachiopoda	14
	1186

In his last volume he gives a brief account of the successive dredging expeditions by which the deeper waters have been explored, from that of the Porcupine in 1869 to those undertaken by private individuals in 1895 and 1896. The species obtained from these greater depths number 625, and 286 of them do not occur in the shallower waters. Thus he makes the total number of shell-bearing Mollusca found on or near the coasts of France to be 1488.

M. Locard may be congratulated on having completed a work of so much labour, and one which cannot fail to be useful to all who are interested in the molluscan fauna of the seas around France. Moreover, as a large number of these species occur also on our own coasts his volumes will also be of service to British conchologists. A. J. J-B.

TOWARDS PERFECTION.

Animal Biology, an Elementary Text-Book. By C. LLOYD MORGAN, F.R.S., Professor of Zoology and Geology in University College, Bristol, and Lecturer on Comparative Anatomy in the Bristol Medical School. Third Edition, revised. 8vo, pp. viii. + 313, with 135 figures. London : Longmans, Green, & Co., 1899. Price 8s. 6d.

This well-known and much-appreciated text-book has been modified a little to meet changes in the requirements of the London University examinations, part of it has been at the same time rewritten, and many illustrations have been added,—the result being that the book, so excellent before, has made a marked step towards perfection. It is one of the soundest books that can be put in the student's hands.

At the same time, we have one general criticism to make,—that the book is even in its improved form distinctly smaller than its title. Prof. Lloyd Morgan has in other works made biologists his debtors by his lucid and balanced exposition of the general problems of biology, and by his original contributions towards their solution ; he has also elsewhere discoursed in a most interesting way on the habits of birds and beasts, and shown how much may be gained from their study ; but of all this there is little trace in the volume before us, which conforms with others in being mainly morphological, differs from most in giving a fair place to physiology, but agrees with almost all in leaving out bionomics. Perhaps the author is right in his reserve, but we doubt it—*for him*. His position, however, is indicated in the sentence,

"With regard to aetiology, the aim will be rather to pave the way for a study of causes by an accurate presentation of facts, than to deal at any length and more than incidentally with the theory of evolution or the doctrine of descent."

J. A. T.

VARIATION-STATISTICS.

Die Methode der Variations-statistik. By GEORG DUNCKER. Pp. 74, with 8 figures. Leipzig: Engelmann, 1899. Price 2 marks, 40 pfg.

As Dr. Duncker explained his position in the last number of *Natural Science*, as Mr. H. M. Kyle discusses the same method in the present number, and as Professor Davenport has published an English guide to the use of the method, we need not waste space by trying to summarise this booklet on the method of variation-statistic. We believe that it is not altogether perfect—it would have been almost a miracle if it had been—but it is a clear statement of the method by one who has used it to good purpose; and we are grateful to Dr. Duncker not only because he has been a pioneer in a fruitful path of investigation, but because he has made it possible for any one with a head on his shoulders to follow in his steps.

NOMENCLATURAL CHANGES IN THE EDENTATA.

Elsewhere we have called attention to certain emendations in the nomenclature of the Chiroptera. A paper by Mr. T. S. Palmer in the *Proc. Biol. Soc., Washington*, vol. xiii. p. 71, suggests others among the Edentata. In an earlier part of the same journal for the current year Mr. G. S. Miller urged that the Armadillos commonly known as *Xenurus* should be designated *Tatoua*, Gray (1865), on account of the preoccupation of the former term. Now Mr. Palmer states that *Tatoua* must itself yield place to the still earlier *Cabassous*, M'Murtrie (1831). Such constant changes (altogether apart from the question whether barbarous names like the foregoing are admissible) are much to be deprecated; and the least an innovator can do is to make sure that he has got hold of the earliest name. Otherwise it is in every way far better to let matters stand as they are.

Mr. Palmer further urges that *Cyclothurus*, for the Pigmy Ant-eater, must give way to *Cyclopes*, Gray (1821); and, what is much worse, that *Uroleptes*, Wagler (1831), must replace its own name (*Tamandua*) for the Tamandua Ant-eater.

UNGER AND ENDLICHER.

Briefwechsel zwischen Franz Unger und Stephan Endlicher, herausgegeben und erläutert von G. Haberlandt. Nut Porträts und Nachbildungen zweier Briefe, pp. 184. Berlin: Borntraeger, 1899. Price 5 marks.

In this publication Prof. Haberlandt has made a most interesting contribution to the history of 19th century Botany. Unger and Endlicher were great men and great botanists, and this careful edition of their correspondence is full of instruction not unmixed with amusement.

A CARBONIFEROUS LANDSCAPE.

Eine Landschaft der Stemkohlen-Zeit. Erläuterung zu der Wandtafel bearbeitet und herausgegeben im auftrage der Direction der Königl. Preuss. geologischen Landesanstalt und Bergakademie zu Berlin. By Dr. H. Potomé. Pp. 40, with 30 figs. and a plate. Leipzig: Borntraeger, 1899. Price with the "Tafel," 25 marks.

To restore the past is one of the most hazardous of tasks, and many have tried it with indifferent results. We have not as yet received the "Wandtafel" referred to above, but if it is in proportion to its size as good as the plate accompanying the pamphlet, it must be very good, for Dr. Potomé has put brains as well as artistic feeling into his picture. It is based upon plastic reconstructions of carboniferous plants, and seems to us so successful that we hope eagerly for more to follow.

L. ANTHROPOLOGIE, TOME X. No. 4.

L. ANTHROPOLOGIE for July and August contains some articles which will be of more than passing interest to those who are following the successive discoveries bearing on the prehistoric civilisation of Western Europe.

(1) Boule and A. Vernière (*L'Abri sous roche du Rond près Saint-Arcons-d'Allier* (*Haute Loire*)) describe the exploration of the rock-shelter of Rond, in the Auvergne district, which has yielded remains characteristic of the Reindeer period. Hitherto no stations of this description have been found in this part of France, at least that could be so dated from their relics. The station of Rond was situated under an overhanging cliff of the volcanic rock so common in the locality. Part of the accumulated *débris* had been previously removed, but sufficient remained to give an area of undisturbed strata of some 12 yards in length by 4 yards in breadth. At some depth in a talus of disintegrated rock and other materials the excavators came upon a black bed of ashes and organic matters, 8 inches thick, in which they discovered several hearths, some bone and flint implements, and osseous remains of various animals, including cave-hyena, reindeer, horse, stag, etc. Both the relics and the fauna are regarded by the authors as characteristic of the Reindeer period.

(2) Dr. Verneau (*Les nouvelles trouvailles de M. Abbo dans la Barma Grande*) recurs to the much debated age of the prehistoric men of Mentone, whose skeletons have, from time to time, been disinterred in the Baoussé-Roussé caves, near that town. Since 1892, when three skeletons were discovered in the Barma-Grande cave, two more have come to light in the same cave (1894), both, however, being at a depth of 5 feet less than the former. One of these skeletons—1·75 in. (about 5 feet 8½ in.) in height and strongly dolichocephalic—had associated with it a few ornaments of perforated teeth and shells. Thus in every respect it closely resembled the three burials discovered in 1892. The second, though only a few feet distant, showed evidence of having been subjected to great heat, as the bones were much carbonised. Dr. Verneau observes that the heat was applied to the body *in situ*, and that consequently it lay either on the surface of what was then the floor of the cave or in a very superficial trench. In the deposits beneath these skeletons portions of the lower jaw of a reindeer and some flint implements were found, which he assigns to the same chronological horizon as the human remains of the later Palaeolithic caves of France. The general conclusion arrived at is, that the two groups were contemporary, the three skeletons having been interred in deep pits in Palaeolithic *débris*, while the two upper ones were deposited at or near what was then the floor of the cave. On the whole he regards these

Mentone skeletons, with their associated relics, as approaching, in their general *facies*, more to Palaeolithic than to Neolithic civilisation.

(3) Salomon Reinach (*Un nouveau texte sur l'origine du commerce de l'étain*) combats the generally accepted opinion that, from the earliest times, the Phoenicians had a monopoly of the tin trade from the Cassiterides to the eastern shores of the Mediterranean until they were dispossessed of it by the Romans. He sets himself, with his usual facility in linguistic researches, to prove the following propositions: (1) that the Phoenician trade in tin has not been attested prior to the year 600 B.C.; (2) that the Phoenicians had not a monopoly of this commerce at any time; and (3) that the Greeks themselves never attributed to the Phoenicians, but to another people, the first commercial intercourse with the Cassiterides. His opinion is that the maritime commerce in tin was discovered by the barbarians of Western Europe, but only long after they became acquainted with the value of the metal, and the regions where it was to be found, through its transmission to the East by land routes. This view he considers to be in harmony with the archaeological evidence, which shows the diffusion of tin, amber, spiral ornaments, the types of bronze weapons and other objects, throughout the whole of Central and North-Western Europe during the Bronze Age.

We have to congratulate our contemporary, *La Feuille des Jeunes Naturalistes*, and the editor, Mr. Adrien Dollfus, on the fact that the November number begins the thirtieth year of the journal's existence. To Mr. Jean Dollfus thanks are due for his liberal assistance, which has made it possible to continue the modest price, and to form the valuable library which is at the disposal of the journal's readers. May *La Feuille* be evergreen, is our sincere wish!

Science for October 20 has an interesting article by Walter T. Swingle, U.S. Department of Agriculture, on the dioecism of the fig on its bearing upon caprification, a paper read before Section G of the American Association for the Advancement of Science at the Columbus meeting.

In the *Irish Naturalist* for October, Dr. Scharff describes an interesting variety of *Limax marginatus*, Müll. (var. nov. *niger*). Specimens were found during a preliminary survey of the MacGillicuddy's Reeks, at an altitude of 2500 to 3100 feet.

The October number of the *Journal of Conchology* contains, amongst other articles, a very useful synopsis of the American species of Diplodontidae, by Professor Dall, and an interesting paper by Mr. Edgar A. Smith, in which fourteen new species of South African marine shells are described and figured.

The Rev. A. H. Cooke contributes an important paper to the *Journal of Malacology* on the "Nomenclature of the British Nudibranchiata," to which is appended a revised classification of the group, based upon Bergh. In the same number Mr Henry Suter has an interesting paper on some New Zealand molluscs (*Paryphanta*, *Rhytida*, *Eudodonta*, *Scalaria*, etc.), and Mr. J. Cosmo Melvill and Mr. Edgar A. Smith contribute illustrated papers describing new species.

The *Naturalist* for November contains, *inter alia*, articles on Lincolnshire Phalangidea, by Rev. E. A. Woodruffe-Peacock; on Lincolnshire Diptera, by the Rev. A. Thornley; on the modern tendency of mycological study, by Mr. Massee; and on the chemistry of the Lakeland trees, by Dr. Keegan.

The *Irish Naturalist* for November contains a long review of Dr. Scharff's "History of the European Fauna," by Mr. G. E. H. Barrett-Hamilton.

In the *Plant World*, No. 11, vol. ii. 1899, the first paper is by R. S. Williams—"Botanical Notes on the way to Dawson, Alaska." It describes

in an interesting itinerary the plants that came under observation. The wealth of mosses and lichens is noticed, and at Dawson city the prevalence of the Ericaceae and the scarcity of Compositae—features common to sub-alpine and sub-arctic situations.

A paper by L. H. Pammel, "Some Ecological Notes on the Muscatine Flora," is a study in hydrophytes, mesophytes, and xerophytes found in certain zones. A continued paper by Mrs. C. A. Creevey, "Plant Juices and their Commercial Values," gives a popular account of methods of extracting juices from plants in various parts of the world, and the physiological effects following the drinking of these juices.

In *Nature Notes* for November, Mr. A. E. Martin discusses editors and annotators of Gilbert White's "Selborne," Mr. F. Coleman discourses on birds and insects as meteorologists, Messrs. C. B. and C. T. Plowright describe Broadland in winter-time, and the Rev. George Henslow gives, for the benefit of young botanists, a beautifully clear statement of the evolutionist view of the origin of species. Naturally, he does not refrain from giving his own interpretation of the factors — the power to vary is called into action by new conditions, and the organs change in conformity or adaptation to these.

The *Westminster Review* for November, which we have received, is full of interesting matter, but the only article directly touching biological questions is a continued criticism of the Contagious Diseases Acts.

In the *American Journal of Science*, No. 44, vol. viii. August 1899, one paper is of interest to the biologist, namely, "Studies in the Cyperaceae," by Theo. Holm, and "On the abnormal development of some specimens of *Carex stipata*, Muhl., caused by *Livia vernalis*, Fitch" (with seven figures in the text drawn from nature by the author).

The diseased condition in question shows itself in the hypertrophied leaves, which become white, except at the tips, while they are flat from base to apex, and are devoid of the usual sheath. The larvae of the parasite were located on the upper surface of the leaves, and although the parasitism was purely superficial from its beginning to end, yet it resulted in the almost complete non-development of stomata, chlorophyll, lignin, and the partial non-absorption of silica. What sort of insect *Livia vernalis* is may be found in works on entomology—at least one would expect so,—for no light is thrown upon it in the article. Can any one suggest what advantage the author finds in using: mestome-bundle for fibro-vascular bundle, mestome-sheath for bundle-sheath, bark-parenchyma for cortex-parenchyma, perihadromatic bundle for—what? pericambium for pericycle, protohadrome for protoxylem, leptome for phloem?

There is no need of bundle after mestome, which is equivalent to the whole term, fibro-vascular bundle. In roots one speaks of cortex-parenchyma, not bark-parenchyma. It is years since pericambium was given up for the better term, pericycle, because the form was apt to be confused with cambium.

We have just received from Dr. L. Bordas, Chef des Travaux Zoologiques in the Faculty of Science at Nancy, a paper from the fifth volume of the *Annales du Musée d'Histoire Naturelle de Marseille*, in which he shows, as we had previously occasion to note in "Fresh Facts," that the respiratory trees of Holothuroids have four functions—respiratory, hydrostatic, plastidogenetic, and excretory.

We have received the first part of Volume III. of the *Transactions and Proceedings of the Perthshire Society of Natural Science*, which contains the following papers:—"List of the Rhynchota of Perthshire," by T. M. M'Gregor and G. W. Kirkaldy; "The Flora of Durdie and Arnbathie," by James Menzies; "The Feathered Tenants of our Dwellings," by Lieut.-Col. W. H. M. Duthie; "On the Protection of Wild Birds in Perthshire," by Col. Campbell;

"A Naturalist's Notes on the Recent Voyage of the 'Blencathra' to the Arctic Regions," by William S. Bruce; "Notes on the Larch Disease," by Alex Pitcaithly. The society is now in its thirty-third year, and seems to be in a very healthy state. It is fortunate in having a splendid county to work in, a fine local museum, an indefatigable curator, and an enthusiastic president.

The Société Neuchateloise de Geographie has been good enough to send us its *Bulletin* (tome xi. 1899, pp. 320). It contains many instructive papers, *e.g.* on the "Préalpes Romandes," by Dr. H. Schardt; on "Persia," by Elisée Reclus; on "Esquimo Skulls," by Dr. Alex. Schenk; on "Skulls from the Valley of the Rhone," by Prof. E. Pitard. A clever geological map illustrates Dr. Schardt's paper.

The November number of the *American Journal of Science* has the following articles:—"Types of March Weather in the United States," by O. L. Fassig; "Some New Minerals from the Zinc Mines at Franklin, N.J., and Note Concerning the Chemical Composition of Ganomalite," by S. L. Penfield and C. H. Warren; "Action of Acetylene on Oxides of Copper," by F. A. Gooch and De F. Baldwin; "Andesites of the Aroostook Volcanic Area of Maine," by H. E. Gregory; "New Mode of Occurrence of Ruby in North Carolina," by J. W. Judd and W. E. Hadden, with crystallographic notes by J. H. Pratt. The scientific intelligence includes an obituary of the late Prof. Edward Orton.

The thirty-ninth publication of the Field Columbian Museum (No. 5, vol. i. of the botanical series) contains an account of *Higinbothamia*, a new genus of Dioscoreaceae, of other new forms in the same order, and of various new Amaranthaceae, by Dr. Edwin B. Uline.

The *Report and Transactions of the South-Eastern Union of Scientific Societies* for 1899 appears with admirable promptness. It contains the reports of various departments, the presidential address by Mr. W. Whitaker on the deep-seated geology of the Rochester district, and numerous papers of interest which we noted at the time of the annual meeting.

The October number of the *Journal of School Geography* contains *inter alia* an interesting article entitled "Life in the Grass Lands," in which a lively endeavour is made to relate human functions in the Steppes with the environmental conditions. The article is extracted from "Man and his Work: an Introduction to Human Geography," by Dr. A. J. Herbertson, of the Oxford Geographical School, and Mrs. F. D. Herbertson, B.A. The book should have been sent for review to *Natural Science*.

The *American Naturalist* for October has the following articles:—"Notes on European Museums," by O. C. Farrington; "On Some Changes in the Names of Fossil Fishes," by O. P. Hay; "The Utility of Phosphorescence in Deep-sea Animals" (to attract food), by C. C. Nutting; "A new Hydroid from Long Island Sound (*Stylactis hooperi*)," by C. P. Sigerfoos; "A Balloon-making Fly," by J. M. Aldrich and L. A. Turley; "Species of *Blissus* in North America," by F. M. Webster; and "Synopsis of North American Astacoid and Thalassinoid Crustacea," by J. S. Kingsley.

Among the articles in *Knowledge* for November we note "Shells as Ornaments, Implements, and Articles of Trade," by R. Lydekker; "Ups and Downs in our Daily Weight," by W. W. Wagstaff; and "Recent Work of the U.S. Biological Survey," by W. M. Webb.

The *Victorian Naturalist* for October contains *inter alia* a discussion of the question "Myxomycete or Mycetozoon?" by D. M'Alpine, and descriptions of some Australian birds' eggs by D. Le Souef.

Among the articles in the *Zoologist* for November there is an interesting diary by Edmond Selous concerning the habits of nightjars, and an account by J. L. Monk of the spawning of *Bombinator pachypus* after two years of captivity in England.

Science Gossip for November contains, among other articles, the following:—"On colouring of Birds' Eggs," by R. J. Hughes, and "On Armature of Helicoid Land-shells," by G. K. Gude. There is also on p. 191 a suggestion well worthy of consideration in regard to co-operative science collections.

The *Journal of the Institute of Jamaica* (vol. ii. No. 6, issued 31st August 1899) contains much interesting matter, a large number of short historical articles, *e.g.* "The Story of the Life of Columbus and the Discovery of Jamaica," by the editor Mr. Frank Cundall, and many scientific papers, of which those by Mr. J. E. Duerden, the enthusiastic and indefatigable curator of the museum, may be especially noted.

Rhodora for November has among its articles one on adventitious plants of *Drosera*, by R. G. Leavitt, and one on the white blackberry, by A. M. Mitchell.

We have also received the following:—

On the physiological perception of musical tone. Being the seventh Robert Boyle Lecture delivered before the Oxford University Junior Scientific Club on 6th June 1899. By Prof. John Gray M'Kendrick, M.D.; LL.D.; F.R.SS. L. and E. Pp. 65. London: Henry Frowde, 1899. Price one shilling net.

A continuation of Acloque's "Faune de France," dealing with birds (pp. 87-336, with 621 figures. Paris: Baillière, 1899, price 5 francs), which sustains the reputation of the previous volumes.

Also a paper by Mr. A. C. Seward, previously noticed in our pages, "On the Structure and Affinities of *Matonia pectinata*, R. Br., with notes on the geological history of the Matonineae," *Phil. Trans.* Series B, vol. cxci. 1899. Pp. 171-209, 4 pls. Price 4s. 6d.

The first Lancashire Sea-Fisheries memoir, "Oysters and Disease, an account of certain observations upon the normal and pathological histology and bacteriology of the oyster and other shell-fish," by Profs. W. A. Herdman and R. Boyce. 4to, pp. 60, 8 pls. London: Philip and Son, 1899. Price 7s. 6d. net.

"The Concilium Bibliographicum in Zürich and its work," by W. E. Hoyle, M.A., and Clara Nördlinger of the Manchester Museum (which is proud to be the possessor of the only complete set of the Zürich cards in England). The paper is reprinted for private distribution from the *Library Association Record*, November 1899; and it is hoped that it may do something to promote increased appreciation of Dr. Field's self-sacrificing bibliographic energy.

"Notes on the Binney Collection of Coal-measure Plants," by A. C. Seward, M.A. Part I. deals with *Lepidophloios*, and Part II. with *Megaloxylon* gen. nov. *Proc. Cambridge Philos. Soc.* x. 1899, pp. 137-174, 2 pls. and 5 figs.

OBITUARIES.

The following deaths have been recently announced :—Dr. Oscar Baumann, the African explorer, at Vienna, on October 12 ; Edward Case, on September 22, an English engineer well known for his method of groyning to prevent encroachments of the sea on the coast ; Cornelio Desinioni, on June 29, in Gavi, Italy, a historian of geography, in his 86th year ; on September 19, in Poturzyca (Galicia), in his 72nd year, Graf Wladimir Dzieduszycki, curator and founder of his Natural History Museum in Lemberg, which is especially rich in birds ; Prof. H. R. Geiger, sometime assistant on the U.S. Geological Survey, at Springfield, Ohio, July 18 ; on July 2, in Regensburg, Wilhelm Geyer, a well-known enthusiast on aquaria ; on July 16, Nikolaus W. Grigorjew, a young phyto-palaeontologist, in Charkow ; Dr. Ragnar Hult, geographer and botanist, at Helsingfors, in his 42nd year ; Paul Janet, the illustrious professor of philosophy at the Sorbonne ; on July 2, in Pará, the botanist, Dr. F. Kuhla, about to start on a botanical expedition to the tributaries of the Upper Amazon ; on June 30, in Stockholm, in his 78th year, Dr. Matts Adolf Lindblad, for twenty years docent in botany in the University of Upsala, known as a mycologist ; in Budapest, in his 55th year, Géza von Mihálkovics, the famous anatomist, professor of anatomy and embryology in the University of Budapest ; on October 16, Dr. Edward Orton, geologist, professor in the Ohio State University, president of the American Association for the Advancement of Science ; on August 9, Mr. William Pamplin, in his 93rd year, the *doyen* of English botanists, who contributed largely to the "London Catalogue of British Plants" ; Lady Prestwich, who recently published a biography of her husband, at Parkstone, on August 26, at the age of 66 ; on August 2, in Buenos Aires, in his 72nd year, Georg Ruscheweyh, a keen lepidopterist ; in Scutari (Albania) the ornithologist Georg Freiherr Schilling von Canstatt ; on October 21, James Simpson, for eighteen years curator of the Anatomical Museum, University of Edinburgh ; W. A. Snow, late instructor in entomology in Stanford University, drowned on October 10, in San Francisco harbour ; on August 3, in Lucerne, Siegfried Stauffer, founder of the Natural History Museum there ; in September, Dr. Carl Gustaf Thomson, curator of the entomological department of the zoological museum in Lund, an authority on Hymenoptera ; at Lakefield (Ontario), Mrs. C. P. Traill, botanist, in her 97th year ; on September 9, in Pöls (Steiermark), the ornithologist Dr. Stephan Freiherr von Washington, in his 41st year ; Dr. Henry Hicks, F.R.S., the distinguished geologist, on November 18, at the age of sixty-two.

CORRESPONDENCE.

A PORTUGUESE PARALLEL TO *NEOMYLODON LISTAI*.

If a mouse may help a lion, may I venture to draw attention to the rather striking parallelism which exists between the discovery of such fresh remains of *Neomylodon* in the dust of a large cavern near Lost Hope Inlet, and Dr. Gadow's find of several skeletons of the Norway Lemming (*Lemmus lemmus*) near Athouguia in Portugal.

Through the kindness of Dr. Gadow I was permitted to announce the discovery at the meeting of the Zoological Society of March 3, 1896 (see *P. Z. S.*, March 6, 1896, pp. 304-306). The circumstances of the case are quite close to those attending the discovery of *Neomylodon*. In both cases the bones were discovered buried under the dust of a cave, in both cases they were surprisingly fresh (the Lemming remains were quite recent, having the skin and the ligaments attached to them), and in both cases the remains found are those of an animal believed to have been long since extinct in the country where they were found.

The present range of the Norway Lemming does not extend south of about 58° 30′ north latitude, while even in Pleistocene times it had been previously unknown from any localities south of England, yet its remains as found in Portugal had the appearance of having belonged to quite recently dead animals.

It would seem then that even in countries where the climate is damp, or certainly not dry, it is possible that, given the aid of a sheltering cave, and of abundance of dry dust, the remains of mammals, both small and large, may be preserved in quite a fresh state for long periods.

G. E. H. Barrett-Hamilton.

Kilmanock, Arthurstown,
Ireland.

BIOLOGICAL ANALOGY AND SPEECH-DEVELOPMENT.

As language (speech) is entirely a human invention—just as chess and piano-playing are—the science of language is not entitled to be classed as a natural science; so it is with much diffidence that I write to you on the subject. But as you published Mr. Henry Cecil Wyld's paper on "Biological Analogy and Speech-development" in your January (1899) Number, may I venture to point out to Mr. Wyld that in his criticism of the fallacy of Professor Paul's reasoning he might possibly mislead as many readers as Professor Paul has.

Mr. Wyld says (p. 48) that "the safest way to think of language is as a habit of body expressing a habit of mind."

The question naturally arises, "Is this a safe way to think of any human invention?"

One might just as well say that the safest way to think of bicycle-riding is as a habit of body expressing a habit of mind, though the cerebration is unconscious—in the case of good riders—just as it is with accomplished speakers in language.

But wherein comes the element of safety pointed out by Mr. Wyld? I should prefer to say that Mr. Wyld's way of thinking of language is a very vague one, and vagueness of thought is not an element of safety in scientific inquiry.

It seems to me far safer for writers on the subject never to lose sight of the fact that language (speech) is a human invention, and has nothing whatever to do with biological analogy or biological phenomena.

Then we shall probably hear a great deal less of the "life" and "growth" of language, its "evolution," its "branches," its "offshoots"; that it is an "organism," that it has "roots," and that there are "mother-languages" and "sister-languages"; and all the rest of the jargon with which philologists becloud their subject.

Philologists will retort that these terms are merely metaphorical: but these metaphors mislead, and have misled many who read books on philology to get a knowledge of what language is. J. I. HAZELAND.

KOBE CLUB, KOBE, JAPAN,
Sept. 6, 1899.

NEW MEXICO BIOL. STATION.

Your note on p. 157 about the N. M. Biol. Station is incorrect. The Biol. Station was conducted by myself and Miss Wilmatte Porter, and concerned itself not at all with geology or anthropology. The students were mostly public school teachers, and occupied themselves with the biology of flowers, particularly the structure of flowers as related to insect visitors. Some work was also done on the mouth-parts of bees, and a few other things. It seemed to me we had as much success as we deserved, and the outlook for the future is encouraging. It is regretted that there is no millionaire available to endow the institution; but the country is full of new and interesting things, and is itself a laboratory better endowed than that of many a wealthy college, so that the naturalist who cannot find profitable occupation must be stupid indeed. The station differs from most others in concerning itself with terrestrial life (not freshwater, or marine), which is especially worth the attention of the student in this region, owing to the desert conditions, resulting in such interesting adaptations.

Your notes on Dr. Judd's paper (p. 89) are interesting. Yesterday I saw a little spider which beautifully mimicked an ant of the genus *Formica*. Now you might say, what for? The ant is a fairly soft, harmless thing, apparently as good meat as the spider. But the great enemy of spiders is a certain wasp, which stores up spiders for its young. Now the wasp *doesn't want ants*, doesn't use that kind of meat. So the spider taken for an ant will escape, though the ant *is* harmless. This couldn't be seen on general principles, one has to know about the customs of the wasp. THEO. D. A. COCKERELL.

MESILLA PARK, NEW MEXICO, U.S.A.

NEWS.

THE following appointments have recently been made:—Dr. Hugo Berger, to be professor of the history of "Erdkunde" in Leipzig; Dr. Edgar R. Cummings, as instructor in geology in the University of Indiana, Bloomington; Dr. E. A. Darling, as bacteriologist to the Cambridge Board of Health, to succeed Dr. G. B. Henshaw; Dr. C. B. Davenport, to fill the post in the University of Chicago left vacant by the removal of Professor Wheeler to the University of Texas; W. L. H. Duckworth, M.A., as lecturer in physical anthropology at Cambridge University; O. Franges, to be professor of pisciculture at the University of Agram; Dr. Sigmund Fuchs, as professor of the anatomy and physiology of domestic animals at the Agricultural Station at Vienna; Dr. K. W. Genthe, as an instructor in zoology in the University of Michigan; Dr. L. C. Glen, as professor of geology in South Carolina College; Dr. Hans Hausrath, to be professor of forestry in the Technical Institute of Karlsruhe; Dr. Henneberg, as docent in anatomy at Giessen; Dr. L. Hiltner, as director of the bacteriological laboratory in the Imperial Health Office in Berlin; V. Hlavinka, as professor of geodesy in the University of Agram; Dr. S. J. Holmes, as an instructor in zoology in the University of Michigan; Dr. E. Jacky, as assistant on the botanical side of the pomological institute in Proskau; J. J. Jahn, to be professor of mineralogy and geology in the Technical Institute in Brunn; Dr. H. S. Jennings, as an instructor in zoology in the University of Michigan; Dr. Stefan Jentys, as professor of agriculture and botany in the University of Agram; Dr. Johannes Christoph Klinge, to be a head botanist and the librarian in the botanic garden of St. Petersburg; S. J. Korshinsky, to be director of the herbarium of the Academy of St. Petersburg; Dr. Alfred Krolopp, as assistant professor of botany in the University of Agram; Dr. Daniel P. MacMillan, to an appointment in connection with the Child-Study Department recently created in connection with the public schools of Chicago—probably the first appointment of this sort, and, we sincerely hope, not the last; Dr. W. D. Merill, as instructor in biology, with special reference to botany, in the University of Rochester; Dr. Merton L. Miller, associate in anthropology in the University of Chicago; Dr. B. Němec, as docent in vegetable anatomy and physiology in the Tschech University of Prag; W. A. Orton, as lecturer on botany in the St. Louis Manual Training School in New York; C. W. Prentiss, as an assistant in zoology at Harvard University; Dr. Eugen Romer, as docent for geography in the University of Lemberg; Dr. J. T. Rothrock, reappointed state commissioner of forestry for the state of Pennsylvania; John Louis Sheldon, as assistant in botany in the University of Nebraska in Lincoln; Dr. E. O. Sisson, as director of the histological laboratory in the recently consolidated medical schools of Keoduk, Iowa; G. Tanfiljew, to be a head botanist in the botanical institute in St. Petersburg; Mr. J. L. Tuckett, Fellow of Trinity College, Cambridge, as an additional demonstrator of physiology; Dr. Velich, as docent in the physiology and pathology of animals in the Tschek University of Prag;

Nikolaus Warpachowsky, as director of the Government Fisheries in Archangel; Dr. Karl Wenle, as docent in geography and ethnology in the University of Leipzig; W. A. Willard, as an assistant in zoology in Harvard University; S. R. Williams, as an assistant in zoology in Harvard University.

Dr. K. Eckhardt, Professor of Physiology at Giessen, has recently celebrated the fiftieth year of his function as a university teacher.

Professor Henry G. Jessup, who has held the chair of Botany in Dartmouth College for twenty-two years, has resigned.

The Council of the Royal Society has adjudicated a Royal medal to Professor William Carmichael M'Intosh for his important monographs on marine animals, his work on the fisheries industries, and his success in establishing the Gatty Marine Laboratory at St. Andrews.

The Council of the Royal Society has adjudicated the Davy medal to Mr. Edward Schunck, F.R.S., for his investigations on madder, indigo, and chlorophyll.

The gold medal of the Highland and Agricultural Society of Scotland has been awarded to Professor Cossart Ewart in recognition of his experiments on hybridisation, telegony, and the like.

Mr. J. J. Lister, University Demonstrator of Comparative Anatomy, and Mr. A. C. Seward, University Lecturer in Botany, have been elected to fellowships in St. John's College, Cambridge, in recognition of their important scientific work.

Dr. G. Elliott-Smith, one of the assistant demonstrators of Anatomy at Cambridge, well known for his researches on the comparative anatomy of the mammalian brain, has been elected a Fellow of St. John's College.

Professor G. Sims Woodhead has been elected to a Fellowship at Trinity Hall, Cambridge.

The degree of M.A. *honoris causa* has been conferred by the University of Cambridge on Dr. W. Somerville, recently elected Professor of Agriculture there.

Grants from the Moray fund of the University of Edinburgh have been made to Professor E. A. Schäfer for the expenses of research on the cerebral nervous system, and to Dr. John Malcolm for experiments on the alterations in bone marrow produced by nucleins and their allies.

At the unveiling of the monument to Johannes Müller, at his birthplace, Coblentz, on October 2nd, Professors Virchow and Waldeyer were the chief speakers. The former pointed out that Johannes Müller was *par excellence* a biologist; the latter referred especially to Müller's influence on the University of Berlin, and on the Prussian Academy of Sciences.

The following gifts and bequests are announced:—D. F. Converse, a mill-owner of Spartanburg, S.C., left one-third of his estate, valued at half a million dollars, to Converse College, an institute which he founded ten years ago in Spartanburg for the higher education of women; by the will of the late Cornelius Vanderbilt, Yale University receives $100,000, and Vanderbilt University half that sum; £20,000 given by Mr. Charles Holcroft for the new Birmingham University, bringing the total endowment up to £315,400; £10,000 was recently subscribed towards enlarging the Durham University College of Science, for which £50,000 is needed.

Vassar College has been promised $25,000 towards a biological laboratory on condition that an equal amount be raised otherwise.

Mr. E. E. M'Millin has given the Ohio Academy $250 for scientific investigations, with a provisional promise that the gift may be annual.

Mr. E. Tuck has given $300,000 to Dartmouth College, U.S.A.; the late Mrs. M. J. Goddard left $60,000 to Tufts College.

The list continues:—$140,000 left by Dr. Calvin Ellis, formerly Dean of the Harvard Medical School, to the University; $90,000 bequeathed by Miss Lucy Ellis, to be added to the fund left by her brother, Dr. Ellis; $50,000 given by an anonymous donor to the University of Pennsylvania for the dormitory system; $25,000 bequeathed to Wesleyan University, Middletown, Conn., by J. H. Sessions; $10,000 given to the Iowa Wesleyan University by ex-Senator James Harlan.

We learn from *Science* that a large collection of water-colour paintings of Japanese fishes by a Japanese artist has been presented to the University of Michigan by Frederick Stears, of Detroit, and is at present on exhibition in the University Museum.

Prof. Starr of Chicago has presented his collection illustrating the ethnography of Mexico to the Folk Lore Society, who have offered to deposit it in the Museum of Archaeology and Ethnology at Cambridge.

£1000 has been bequeathed by the late Mr. C. P. Daly to the American Geographical Society for the foundation of a medal to be awarded for distinguished services in geography.

The *American Naturalist* notes that the sons of the late Prof. J. Marcou have presented his geological library to the American Museum of Natural History in New York.

Over fifty students, says the *American Naturalist*, attended the Coldspring Harbour biological laboratory during the summer of this year.

It is noted in *Science* that the expenses of the University of Chicago for printing and publishing during the academic year ending June 30, 1899, were over $44,000, while the receipts were only $17,000. It is probable that no other University supports its publications with such liberality.

It is stated in the *Scientific American* that the number of women in attendance at the German Universities during the summer semester of 1899 was 355. There were 179 at Berlin, 45 at Bonn, 27 at Breslau, 29 at Göttingen, 13 at Heidelberg, and 19 at Halle. The University at Strasburg has just decided to admit women to its courses. Hitherto it has closed its doors to women, but now there is no German university where they may not pursue their studies.

There are fifteen Universities in France, with 27,080 students, of whom 12,059 belong to Paris. The total expenditure is 13,859,500 francs, of which 10,524,200 has each year to be found by the State.

The *Scientific American* notes that last year the regents of the University of California sent out invitations to the architects of Europe and the United States to participate in a competition whose object was to secure the best possible plans for new buildings for the university. A careful programme was outlined, and in deference to European architects, Antwerp was selected as the city where the first competition should be held, and 101 plans were received from architects in every country in Europe and from the United States as well. A representative international jury passed on the plans.

On September 8 they announced that the plan of M. E. Bénard, of Paris, was successful and would receive the $10,000 prize. Mrs. Phoebe A. Hearst gave $100,000 for defraying the necessary expense of the competition; she has also promised to bear the cost of some of the buildings. The whole scheme calls for $20,000,000.

It is good news that the Liverpool Marine Biology Committee has published the first of a series of Memoirs on typical British marine plants and animals, edited by W. A. Herdman, D.Sc., F.R.S. No. 1 is on *Ascidia*, by Professor W. A. Herdman, D.Sc., F.R.S. It has 60 pp. and 5 plates, and costs 1s. 6d.

It is hoped that this series of special studies, written by those who are

thoroughly familiar with the forms of which they treat, will be found of value by students of Biology in our laboratories and in marine stations, and will be welcomed by many others working privately at marine natural history.

It is proposed that the forms selected should, as far as possible, be common Irish Sea animals and plants, of which no adequate account already exists in any text-book.

The first three Memoirs will be issued before the end of 1899, and others will follow, it is hoped, in rapid succession :—Memoir I. *Ascidia*, W. A. Herdman ; Memoir II. Cockle, J. Johnstone ; Memoir III. *Echinus*, H. C. Chadwick ; *Dendronotus*, J. A. Clubb ; *Zostera*, R. J. Harvey Gibson ; *Halidrys*, C. E. Jones ; *Codium*, R. J. H. Gibson and Helen Auld ; Diatoms, F. E. Weiss ; *Gigartina*, O. V. Darbishire ; *Alcyonium*, S. J. Hickson ; Plaice, F. J. Cole and J. Johnstone ; *Botrylloides*, W. A. Herdman ; Cuttle-fish, W. E. Hoyle ; Ostracod, Andrew Scott ; *Patella*, J. R. Ainsworth Davis ; *Calanus*, I. C. Thompson ; *Actinia*, J. A. Clubb ; Polyzoon, Laura R. Thornely ; Calcareous Sponge, R. Hanitsch ; Porpoise, A. M. Paterson ; *Arenicola*, J. H. Ashworth ; Oyster, W. A. Herdman.

The editor acknowledges a welcome donation of £100 from Mr. F. H. Gossage of Woolton, which has met the expense of preparing the plates in illustration of the first few memoirs, and so has enabled the Committee to commence the publication of the series sooner than would otherwise have been possible.

The Committee desire to intimate that no copies of these memoirs will be presented or exchanged, as the prices have been fixed so low that most of the copies will have to be sold to meet the cost of production.

The memoirs may be obtained, post free at the net prices stated, from the Hon. Treasurer, Mr. I. C. Thompson, 53 Croxteth Road, Liverpool ; Professor Herdman, University College, Liverpool ; or the Curator, Biological Station, Port Erin, Isle of Man.

The Millport Marine Biological Station issues an appeal for a sum of £300, required for the pumping and circulating apparatus. The fund for this is to be kept independent of the general maintenance accounts. The Millport Marine Station has the distinction of being a scientific institution founded and maintained by private liberality on the part of persons interested in the advancement of science, and it will be a matter for congratulation if, before the Glasgow meeting of the British Association in 1901, its equipment is complete in the important department to which this appeal has special reference.

We read in *Science* that teachers in Philadelphia public schools are now allowed to take their classes for a half-day once or twice a year to the Zoological Gardens and Fairmount Park, the visit counting as part of the regular class duties.

Science reports some of the general results of the third Princeton expedition to Patagonia, conducted by Mr. J. B. Hatcher and his assistant Mr. O. A. Paterson.

(1) A good preliminary geological survey of that part of southern South America lying between the Andes on the west and the Atlantic on the east, and between the Straits of Magellan and the forty-seventh parallel of south latitude, sufficient to serve as a basis for a geological map of the region.

(2) Very extensive and complete collections of fossils from all the horizons known to that region, with the exception of the *Pyrotherium* beds.

(3) The discovery of four distinct and previously unreported geological horizons.

(4) A collection of more than a thousand skins and skeletons of recent birds and mammals.

(5) Extensive collections of the freshwater, terrestrial, and littoral invertebrates.

(6) Botanical collections, especially of the mosses, hepaticae, and flowering plants, not including the grasses and sedges.

(7) A large series of photographs illustrating the geology and physical geography of Patagonia.

The geology will be treated of by Mr. Hatcher, the Tertiary invertebrates by Dr. Ortmann, the fossil vertebrates by Messrs. W. B. Scott and Hatcher, and the recent birds by Mr. W. E. D. Scott.

At the meeting of the Biological Section of the New York Academy of Sciences on October 9, Professor H. F. Osborn gave an account of the exploration of the American Museum party in Southern Wyoming, which resulted in the discovery of Dinosaur remains; Professor E. B. Wilson reported the discovery of females of *Polypterus* in Egypt, but with unripe ovaries, and the rediscovery of the branchiate Oligochaete *Alma;* and Professor Dean reported finding on the Californian coast freshly hatched young of *Bdellostoma*, and many stages of *Chimaera collieri*.

Professor Franz von Höhnel of Vienna has undertaken a botanical exploration in Brazil.

In *Nature* for November 9 Mr. John C. Willis gives an account of the facilities now available in Ceylon for botanical research.

We learn from the *Scientific American* that the Duke of Abruzzi has found an important mistake in the last map of Franz Josef Land. He says that Cape Flora is really ten geographical miles east of the post assigned on Jackson's map. The map of Payer was riddled by Jackson, who complained of its inaccuracies, but he has himself assigned the wrong position to his own camp.

On the Skeat expedition Mr. Evans found several species of *Peripatus* in Kalantan. As the distribution of this animal is of peculiar interest we may note also that in 1886 Mr. R. Horst recorded its occurrence from East Sumatra on the other side of the Malaka Strait. See *Nature*, November 9, 1899, p. 31.

Science reports that Mr. R. E. Snodgrass, assistant in entomology in Stanford University, and Mr. A. H. Heller, have returned from a successful ten months' collecting trip to the Galapagos Islands. The collections of birds, fishes, insects, and spiders, are said to be large.

In the judgment of Major Ronald Ross, who has now returned from Africa, the future of the west coast will be assured as soon as the colonial authorities take steps similar to those now in operation in Sierra Leone, to destroy the virulent mosquito.

Geheimrath Prof. von Zittel of München is arranging to send a scientific expedition to Patagonia.

We learn from *Science* that Mr. O. F. Cook of the Division of Botany, U.S. Department of Agriculture, has been sent to examine the plant products of Puerto Rico in reference to the possibility of introducing new and useful tropical plants into the island. He is accompanied by Mr. G. N. Collins as photographer, and Mr. G. P. Gall sent by the Smithsonian Institution to collect material for the National Herbarium.

Nature reports that another British exploring expedition to Abyssinia has been arranged, and will leave England at once for nine months. The objects are science and sport.

The annual conversazione of the Geologists' Association, London, was held on November 3, and was fairly well attended in spite of the inclement weather. Among the more striking exhibits were a fine series of concretionary structures brought together by Dr. G. Abbott; the skin and skull of *Neomylodon listai* lent by the La Plata Museum, and shown by A. Smith Woodward; a series of pebbles from Derbyshire compared with a corresponding series from

the London basin, and taken by Mr. A. E. Salter as evidence for a former river-connection between the two areas. From Derbyshire also, as a result of the long excursion, came a collection of Carboniferous limestone fossils made by Miss M. C. Foley, as well as various photographs. The Carboniferous limestone of the Isle of Man had yielded to Miss C. Birley a good set of Cephalopods. W. H. Chadwick and P. Emary showed Graptolites from the Wenlock shales and Llandeilo beds of Builth and St. David's. English and Indian Trigonias were shown by Prof. J. F. Blake, and other fossil collections by H. W. Burrows, W. F. Gwinnell, and F. R. B. Williams. The last mentioned also exhibited William Smith's Geological Sections from London to Snowdon. In contrast the latest maps of the Geological Survey were shown by Sir Archibald Geikie. A. S. Foord exhibited photographs of the striking frescoes in the Historical Museum at Moscow, showing scenes of Russian life in the Stone Age and in the tenth century. Wind-worn pebbles from England, Esthland, New Zealand, Bohemia, and Egypt were shown by F. A. Bather and Rev. Prof. T. G. Bonney, the latter also sending schistose Jurassic rocks from Nufenen and Scopi in the Alps, and Pre-triassic Alpine Schists from the Val Piora. These and many other exhibitors showed that the activity of the Association was in no way diminishing.

Mr. P. L. Sclater, on his recent visit to South Africa, gave an address to the South African Philosophical Society, in which he pointed out the desirability of establishing a Zoological Garden in Cape Town. It was doubtless towards this end that Mr. Rhodes sent his lion.

The lectures to be delivered before the Hull Scientific and Field Naturalists' Club during the rest of the winter session, 1899-1900, include the following:—"Natural History Notes in North Wales," by the President, R. H. Philip; "Symbiosis—A study in Plant Partnerships," by Mr. J. E. Robinson; "Cyclone and Cloud—A study of English Weather," by Mr. C. H. Gore, M.A.; "Solar Eclipses, with special reference to that of May 28, 1900," by Rev. H. P. Slade; "Wild Fowling and Decoying," by Mr. T. Audas, L.D.S.; "Economical Illumination," by Dr. J. T. Riley, A.R.C.Sc.I.; "What is a Species?" by Dr. H. H. Corbett, M.R.C.S. (of Doncaster). In January the club will hold an exhibition and conversazione.

The *Scientific American* notes that "it is not often that specimens in museums are destroyed by reason of being eaten, but it seems that in one of the Southern States a negro clay-eater who was employed as a scrubwoman devoured some of the finest specimens of kaolin on exhibition at the State Geological Museum. The State geologist found that five blocks of clay which were very highly valued on account of their purity were missing, and upon examining some of the other specimens he found on them the impression of teeth. Detectives were set to work on the case, and the negress employed to scrub the marble floors was accused of taking the specimens. The woman appears to have a mania for eating clay, and she had been indulging her strange appetite for some time."

Knowledge notes that a collection illustrating changes due to domestication has been begun at the British Museum (Natural History). A number of interesting stuffed specimens and skeletons have been placed on exhibition in the gallery of British Zoology.

The *Scientific American* notes that the city of New York has made an appropriation of $10,000 for the purpose of making a great relief map of the whole city. The map will be about 50 feet square, and will show all the important buildings. Buffalo will also be represented in probably the same manner, with a relief map which will show Niagara Falls and its power plants.

We learn from the *Scientific American* that the U.S. Department of Agriculture desires an ornithological clerk who must have an excellent knowledge of orni-

thology and mammalogy, and his examination will include a practical test in the identification of specimens of birds and mammals. In fact, these two subjects count 70 per cent in the examination to be held. The person who succeeds in passing will be placed on the eligible list, and if selected will receive the munificent salary of $660 per annum.

The *American Naturalist* notes that an Entomological Society has been founded, with Dr. E. F. Felt, State Entomologist, as president.

The *Scientific American* notes that the executors of the late Prof. O. C. Marsh have sold his valuable collection of orchids, but the prices were extremely low. It seems a pity that a collection of this size and importance was not procured intact for some botanical garden.

Prof. A. L. Herrera has been kind enough to send us a small sample of calcareous soap mixed with albumen and peptone, which when warmed on the slide with water will move and fill with vacuoles, without, however, giving off any pseudopodia.

We learn from the *Scientific American* that for several years attempts have been made at Omaha and Los Angeles to hatch the eggs of the ostrich artificially, but so far we believe their attempts have been unsuccessful, the difficulty being the application of moisture. Now, however, an ostrich farm in Florida can boast of the first incubator-hatched ostrich in the United States. The incubation required forty-one days of careful watching, the thermometer was kept at 110° and the moisture was applied at intervals.

On November 21 the Edinburgh Town Council gave a favourable reception to an influential deputation who appeared in order to urge the Corporation to give their influence towards the promotion of the movement for the establishment of a zoological garden in Edinburgh.

The Mortimer Museum of Antiquities at Driffield, Yorkshire, contains a very good local collection. Its owner has offered it to the East Riding County Council for half its value, the value to be decided by two referees, one to be appointed by the Council and the other by Mr. Mortimer. We understand that the Council has, on legal grounds, some hesitation in accepting this generous offer; but we hope that it will be bold enough to follow the example of other County Councils, as otherwise, on Mr. Mortimer's death, the collections will be sold and scattered.

INDEX

PAGE

Abbott, A., and Arthur Key, "Progressive Lessons in Science" (*Review*), 365
Abbreviations Criticised, . . . 85
Aclogue, A., "Faune de France-Mammifères" (*Review*), . . . 297
African Fauna, ... 142
Agricultural Progress, . . . 359
Algae new to Britain, . . . 162
Allen, E. J., on the Sea-floor, . . 164
Allman on Symmetry, . . . 99
Alpine Characters in Plants, Artificial Production of, . . . 60
,, Climbing, ... 298
,, Guide, ... 146
,, Plants, ... 109
Alps, The Flora of the, by Alfred W. Bennett, ... 109
American Mammals, Notes on, . 311
,, Plant Notes, . . . 313
,, Species of Peripatus, . 84
Amphibians, Thyroid and Thymus of, 287
Anal Glands of Beetles, . . . 141
,, ,, Dytiscidae, . . 60
Anderson, R. J., Some Considerations concerning Symmetry, . 97
Animal Mind, The, . . . 1
Annelid from the Devonian, . . 168
Antarctic in the Arctic, . . . 319
Anthropology, . . . 219, 456
Appendicularia, ... 213
Applied Geology, ... 64
Aquatic Plants, ... 245
Arctic Birds, ... 366
,, Europe, Devonian Rocks of, . 141
,, Exploration, . . . 318
Ascaphus, ... 89
Asexual Nuclear Fusions, . . . 239
Assimilation in Plants, . . . 251
Asterionella, ... 386
Aurelia aurita, ... 356
Australian Flora, Suggestions upon the Origin of the, by Spencer Moore, . 198, 274
,, Museum, The Difficulties of the, . . . 317
Autogamy in Primulaceae, . . 441

Badenoch, L. P., "True Tales of the Insects" (*Review*), . . . 68
Balance of Nature, ... 309
Ball, John, "Hints and Notes for Travellers in the Alps" (*Review*), . 146

PAGE

Bard, L., "Spécificité cellulaire, ses conséquences en biologie générale" (*Review*), ... 291
Barrett-Hamilton, G. E. H., Notes on the Habits of the Northern Fur Seal, ... 17
Bather, F. A., Fauna of the Sound, . 263
,, ,, A Zoologist on the Principles of Science, 423
Baumhauer, H., "Classes of Crystals" (*Review*), ... 444
Bedford, F. P., Stray Impressions of the Marine Invertebrates of Singapore and neighbouring Islets . 130
Beer, Rudolf, on the Multinuclear Cells of some Grasses, . . . 435
Beeren Eiland, ... 316
Beetles in Self-Defence, . . . 141
Bennett, Alfred W., The Flora of the Alps, ... 109
Berthelot's Experiments, . . . 384
Binet, Alfred, "The Psychology of Reasoning, based on Experimental Researches in Hypnotism" (*Review*), ... 225
Bipolarity and Bryozoa, . . . 6
,, more about, . . . 7
"Bird Life in an Arctic Spring." The Diaries of Dan Meinertzaghen and R. P. Hornby (*Review*), . . 366
Birds, Cries and Call-Notes, . . 148
,, Smell in, ... 140
Bivalves, Cephalic Eyes of, . . 61
Blatchford, T., "Geology of the Coolgardie Goldfield" (*Review*), . . 229
Blue Coral, Colouring Matter of, . 8
Bolton, "Report of Proceedings, at the Tenth Annual Meeting of Museums' Association" (*Review*), . . . 65
Bonney, J. G., The Original Rock of the South African Diamond, . 173
Bordage on Regeneration, . . . 320
Botanical Biography, . . . 11
Brain, Comparative Anatomy, . . 143
Branchial Respiration in Millipedes, 355
Breise, B. B., "On Inhibition" (*Review*), ... 360
Brightwen, E., "Rambles with Nature Students" (*Review*), . . . 145
British Botany, ... 162
,, Mammals, ... 388
,, Museum, Trustees of the, . 3

PAGE

"Britten and Boulger Biographical Index of British and Irish Botanists" (*Review*), ... 11
Brown, H. T., on Elaboration in Plants, 251
Bryozoa and Bipolarity, ... 6
Burdon-Sanderson on Movement of Plants and Animals, ... 14
Butterflies' Wings, ... 294

Carbon, Fixation of, by Plants . . 251
Carpenter, G. H., "Insects: their Structure and Life. A Primer of Entomology" (*Review*), . . 297
Cell as a Unit of Organisation, . . 395
Cellular Immunity, Eel Poison and, . 323
Cephalic Eyes of Bivalves, . . . 61
Cereal Rust Problem, The,—Does Eriksson's mycoplasma exist in Nature? by George Massee, . . 337
Chelonia, Variation in, ... 224
Chemistry, History of, ... 147
,, Modern, ... 364
Child-study, ... 227
Chlorophyll, ... 252, 288
Clamps in Animals, ... 355
Claus, Obituary of, ... 232
Coccidology, ... 295
Cold, Influence of, on Development, 286
Colombian ore, ... 294
Comparative Chemistry of our Forest Trees, The, by P. Q. Keegan, ... 53
,, Psychology, ... 1
Complementary Males, . . . 14, 163
Coolgardie, Geology of, ... 229
Copepods, how they swim, . . . 356
Coppinia, ... 249
Correns, C., "Untersuchungen u. d. Vermehrung der Laubmoose durch Brutorgane und Stecklinge" (*Review*), ... 451
Correspondence, . . . 236, 371, 462
Cossmann, P. N., "Elemente der empirischen Teleologie" (*Review*), . 291
Crystallography, ... 443, 444
Cytology, Course of, ... 228

Darmstadt Museum, ... 167
Darnel, The Poison of, ... 249
Darwinism, ... 361
Darwin's Doggedness, ... 93
Dawson, C., and Woodhead, S. A., Problem of Honeycomb, . . 347
De Vries on Inheritance, ... 240
Delage and Hérouard, "Traite de Zoologie Concrete" (*Review*), . 445
Dendy on the Parietal Eye, . . 87
Devonian, An Annelid from the, . 168
,, Rocks of Arctic Europe, . 141
Diamonds, ... 173
Diaphragm, ... 288
Diastataxy, ... 321
Dicyema, ... 140
Difflugia, experiments on, . . 213
Digestion in Fishes, ... 356
Diplospondyly, ... 215
Diseases of Plants, ... 289
Dispersal of Seeds, ... 94
Driesch on Morphogenesis, . . 243
Duckmoles, how the Young get Milk, 140

PAGE

Duncker, G., Variation-Statistics in Zoology, ... 325
,, ,, Die Methode der Variations-Statistik . 455
Dunes, ... 10
Dytiscidae, Anal Glands of, . . 60

Echinoderms at the British Museum, 4
Edible Medusae, ... 60
Eel Poison and Cellular Immunity, . 323
Egg within Egg, ... 61
Elaboration in Plants, ... 251
Elsden, J. V., "Applied Geology," Part II. (*Review*), ... 64
Embryology, Contribution to Experimental, ... 440
Enterochlorophyll, ... 288
Enteropneusta, ... 224
Entomological Exhibition, . . 90
Entomology, ... 222, 297
,, Popular, ... 68
Equilibration, The Problem of, . . 356
Eriksson's Mycoplasma, Does it exist in Nature, the Cereal Rust Problem, by Geo. Massee, ... 337
Errera on Inheritance, ... 90
Ethics and Meteorology, ... 49
Evolution, F. W. Headley on, by R. F. Licorish, ... 46
Evolution, Discussion of, . . . 227
,, Doctrine denied, . . 81
,, Influence of Nervous System in, . . . 253
Excavations on Puffin Island, by Philip J. White, ... 42
Excretion in Molluscs, ... 61
,, Physiology of, . . . 367

Fauna of Europe, History of, . . 357
,, of Frog Spawn, ... 355
,, of the Sound, by F. A. Bather, 263
,, of Wells, ... 140, 288
Ferment, a Reducing (in Animals), . 355
Fermentation, ... 290
Fertility inherited, Is, ... 15
Fish, concerning an ancient, . . 440
Fisheries, ... 228
Fishes, Digestion in, ... 356
,, Linné's Types, Specimens of, 396
,, Memory in, ... 440
Flatau, E., and S. Jacobsohn, "Handbuch der Anatomie und vergleichenden Anatomie der Centralnervensystems der Säugethiere (*Review*), . 143
Flora of Africa, ... 364
,, of the Alps, by Alfred W. Bennett, ... 109
,, of Australia, ... 198
,, of Britain, ... 226
,, of Sand Dunes, ... 10
Flower, Sir W. H., ... 151
Fluted Scale, ... 310
Forest Trees, Comparative Chemistry of our, by P. Q. Keegan, . . 53
Fossil, A false, ... 215
Foster, Sir Michael, on integration in Science, . . . 161
,, ,, on the Scientific Spirit, . . 237
Freezing Eggs without killing them . 61

PAGE

Frenkel, Prof., "Les Fonctions Rénales" (*Review*), . . . 367
Frog Spawn, Fauna of, . . . 355
Fungi, Ferments in, ... 391
,, new to Britain, . . . 162

Galway Natural History Museum, . 11
Gases, liquefaction of, . . . 147
Gellé, M. E., "L'Audition et ses organes" (*Review*), . . . 63
Geology, Experimental, . . . 288
Gerland and Traumuller, "Geschichte der Physikalische Experimentier-Kunst" (*Review*), ... 68
Goeldi on a Dawn-spider, . . . 13
Grasses, on the Multinuclear Cells of some, by Rudolf Beer, . . . 434
Green Amoebae, ... 85
,, Pigments, ... 288
Green, E. E., "The Coccidae of Ceylon" (*Review*), ... 295
Green, J. R., "The Soluble Ferments and Fermentation" (*Review*), . 290
Groom on Nuclear Fusions, . . 239
Groos, Karl, "Die Spiele der Menschen" (*Review*), ... 450
Grote, A. R., "Specialisations of the Lepidopterous wing: The Parnassi-Papilionidae," Parts I. and II. (*Review*), ... 294
Ground-Sloth of Patagonia, The supposed existing, by A. Smith Woodward, ... 351
Gruber on Green Amoebae, . . 85
Gruvel on Complementary Males, . 14
Gypsy Moths, ... 310

Haberlandt, G., "Briefwechsel zwischen Franz Unger und Stephan Endlicher herausgegeben und erläuert" (*Review*), . . . 455
Häcker, V., "Praxis and Theorie der Zellen- und Befruchtungslehre" (*Review*), ... 228
Hag, alleged Parietal Eye of, . . 287
Headley, Mr. F. W., on Evolution, by R. F. Licorish, ... 46
Hearing, Sense of, ... 62
Heredity, ... 90, 92
,, Nucleolus in, . . . 240
,, Proper and Improper View of, ... 92
Herrera, A. L., A Theory of Sleep, . 134
,, and Vergara Lope on Plateau Life, . . 169
,, on Nomenclature, . 94
Herrmann, O., "Steinbruchindustrie und Steinbruchgeologie" (*Review*), 63
Hertwig, O., "Die Elemente der Entwickelungslehre des Menschen und der Wirbelthiere" (*Review*), . . 445
Hibernating Swallows, . . . 213
Hillyer, H. W., "Laboratory Manual" (*Review*), ... 453
History of Chemistry, . . . 147
,, of Experimental Physics, . 68
Hoernes, R., "Paläontologie" (*Review*), ... 447
Honeycomb, Problem of, by C. Dawson and S. A. Woodhead, . . 347

PAGE

Hopkins Seaside Laboratory, . . 247
Hoyle, W. E., "The Manchester Museum, Owens College. General Guide to the Natural History Collections" (*Review*), ... 148
Hrdlicka, A., "Anthropological Investigations on One Thousand White and Coloured Children of both Sexes" (*Review*), . . . 227
Hutton, F. W., "Darwinism and Lamarckism, Old and New" (*Review*), ... 361
Hymenoptera, Morphology of the Sting in, ... 244
Hypoderma bovis, ... 288

Ichthyosaurus at Home, . . . 171
Immunity acquired before Birth, . 322
,, Cellular, ... 323
Influence of Cold on Development, . 61
Inheritance, a note on, ... 393
,, Facts of, . . . 214, 286
,, in Parthenogenetic Multiplication, . . 287
,, of Acquired Characters, 90
,, of Fertility, . . . 15
,, of Longevity, . . 241
,, of Malformations, . . 240
,, of Variations, . . 214
Insects and Tobacco, ... 170
Instincts of Young Chicks, . . 2
Integration in Science, . . . 161
Isopoda, ... 292

Jackson, C. L., "The Lancashire Sea Fisheries" (*Review*), . . . 228
,, on Recapitulation, . 82
Jellyfish, Variations in, . . . 356
Judd on Protective Adaptation, . 89
Jurassic Nautili, Sexual Dimorphism in, ... 215

Kainogenesis 9
Keane, A. H., "Man Past and Present" (*Review*), . . . 219
Keegan, P. Q., The Comparative Chemistry of our Forest Trees, . 52
,, ,, Trees in Winter, . 399
Kirby, F., "Sport in East Central Africa" (*Review*), ... 142
Kükenthal, W., "Leitfaden für das Zoologische Prakticum" (*Review*), . 144
Kyle, H. M., An Extension of the Method of treating Variations, with Examples and certain Conclusions . 410

Lacepède, "Tableaux des Mammifères et des Oiseaux, 1799," . . 406
Lake and Pond, What is the difference between, ... 60
Lamarckism, ... 361
Land-winning by Plants, . . . 245
Leech, Impregnation in, . . . 61
Leport on Evolution, . . . 81
Lewis, W. J., "Crystallography" (*Review*), ... 443

PAGE

Licorish, R. F., The Influence of the Nervous System in Organic Evolution, . . 253
,, Mr. F. W. Headley on Evolution, . . . 46
Linnaean Names, ... 229
Linné's Type Specimens of Fishes, . 396
Liversidge on Blue Coral, . . . 8
Locard, M., "Lec coquilles marines des côtes de France" (*Review*), . 453
Longevity, Inheritance of, . . 241
Lönnberg, Einar, *see* Bather, . . 263
Luminous Organs, ... 288
Lygodactylus picturatus, . . . 355

Mackintosh, R., "From Comte to Benjamin Kidd" (*Review*), . . 449
Malformations, Inheritance of, . . 240
Mammalian Red Blood Corpuscles, Nuclei of, ... 144
Mammals of France 297
Manx Slates, Age of the, . . . 214
Marine Invertebrates of Singapore and neighbouring Islets, Stray Impressions of the, by J. F. Bedford, 130
Massee, G., "Text-book of Plant Diseases caused by Cryptogamic Parasites" (*Review*), . 289
,, The Cereal Rust Problem —Does Eriksson's Mycoplasma exist in Nature" (*Review*), . 337
Matschie, P., "Die Fledermäuse der Berliner Museums fur Naturkunde" (*Review*), ... 450
Mauna Loa, Eruption of, . . . 248
Meek on Growth of Muscle, . . 245
Memory, ... 146
Mesenchytraeus solifugus, . . 287
Mesozoa 445
Meteorology and Ethics, . . . 49
Meunier, S., "Geologie Experimentale" (*Review*), ... 298
Mexican and Central American Squirrels, ... 13
Mice, Alleged New, from St. Kilda, . 387
Microscopic Vivisection, . . . 213
Microscopy, ... 227
Milk, How Young Duckmoles get, . 240
Mill, H. R., "The International Geography" (*Review*), . . . 446
Millipedes, Respiration in, . . 355
Mills, Wesley, reviewed, . . . 1
"Missouri Botanical Garden Report" (*Review*), ... 447
Mitchell, P. C., on Diastataxy, . . 321
Molluscan Liver so-called, . . 397
Molluscs, Excretion in, . . . 61
Mongoose in Jamaica, . . . 310
Monocotyledons, ... 92
Moore, S., Suggestions upon the Origin of the Australian Flora, . 199, 274
Moore on Tanganyika, . . . 16
Morgan, C. L., "Animal Biology" (*Review*), ... 454
Morphogenesis, ... 243
Morphology of the Sting in Hymenoptera, ... 244
Mosses 451

PAGE

Moxly, J. H. S., "The Tides simply explained, with Practical Hints to Mariners" (*Review*), . . . 443
Multinuclear Cells of some Grasses, by Rudolf Beer, ... 434
Münsterberg on Science and Conduct, ... 95
,, "Psychology and Life" (*Review*), . . . 95
Muscle, Factors in the Growth of, . 245
Museum of Australia, . . . 317
,, of Darmstadt, . . . 167
,, Dundee, ... 171
,, of Manchester, . . . 148
,, Stuttgart, ... 171
Museums Association, ... 65
Myxine, ... 287
Myzostoma, ... 163

Natural History Museum, Accessions, 5
,, ,, ,, Biological Corner of a, 396
,, Science in Australia, . . 7
,, Selection, The Scope of, by J. L. Tayler, . . 114, 183
Nature, Disturbing the Balance of, . 309
,, Studies, ... 381
Nemertean, new Pelagic, . . . 441
Neomylodon listai, ... 351
Neomylodon, more Traces of, . . 440
Nephrite, ... 83
Neptuneopsis, ... 393
Nervous system, ... 253
,, in Organic Evolution, The Influence of the, by R. F. Licorish, . 253
Nest of a Tree-swift, ... 9
Newman, G., "Bacteria" (*Review*), . 452
News, . . 72, 154, 233, 302, 373, 404
Nichols, H. W., "The Ores of Colombia, from Mines in Operation in 1892" (*Review*), ... 294
Nomenclature, Reformed, . . . 94
Northern Fur Seal, Notes on the Habits of the, by G. E. H. Barrett-Hamilton, ... 17
Northern Monocotyledons, Colours of, 92
Notes of Birds, ... 148
Notochordal Canal in Man, . . 213
Nuclear Fusions, ... 239
Nuclei of Mammalian Red Blood Corpuscles, ... 141
Nucleolus in Heredity, . . . 240
Nucleus, Rôle of the, . . . 440

Obituaries, . . . 70, 151, 232, 301
Rudolf Leuckart, Sir W. H. Flower, Carl Claus, John Cordeaux, George Dowker.
Onychoteuthis, ... 4
Ordeal by Fire, ... 84
Organic Evolution, Influence of the Nervous System on, by R. F. Licorish, 253
Organisation, The Cell as a Unit of, . 395
Orthoptera, Regeneration in, . . 320
Orton on Progress of Science, . . 247
Ostwald, W., "Grundriss der Allgemeinen Chemie" (*Review*), . . 364

PAGE

Outlines of Zoology, ... 144
Ovum in Ovo, ... 61

Palaeolithic Man in Scotland, . . 169
Palaetrochis 215
Parietal Eye, ... 87
,, ,, has the Hag a, . . 286
Parnassi-Papilionidae, ... 294
Parthenogenesis in a Sea-Urchin, the Production of, ... 382
Pathological Pigeon, ... 214
Peach, B. N., and Horne, J., "Memoirs of the Geological Survey of United Kingdom: The Silurian Rocks of Britain" (*Review*), . . 216
Pearson, K., "Mathematical Contributions to the Theory of Evolution" (*Review*), ... 15
Pearson on Inheritance of Fertility, . 15
,, on Longevity, ... 241
Penaeus, Statocysts of, . . . 356
Peripatus, American Species of, . 84
Phisalix on Viper's Venom, . . 324
Phyllomedusa hypochondrialis, . 355
Phylogenetic Senescence, . . . 397
Phylogeny of the Rodents, . . 383
Phylogeny of Rust, ... 389
Physiology of Movement, . . . 14
Pigeon, Abnormality in, . . . 214
Plant Morphology, Studies in, . . 398
Plants protected against Snails, . 3
Plateau Life, ... 160
Play, ... 450
Pleurococcus, ... 238
Poison of Viper, ... 324
Polemics and a Parasite, . . . 163
Pond, What is the difference between a Lake and a, ... 60
Pontifical Plant, ... 392
Potomé, H., "Eine Landschaft der Stemkohlen-Zeit" (*Review*), . . 456
Poynting on Scientific Explanations, 250
Practical Cytology, ... 228
,, Zoology 145
"Praktische Anleitung zur Analyse Silicatgesteine" (*Review*), . . 444
Primulaceae, Autogamy in, . . 441
Problem, An unsolved, ... 251
"Proceedings of the Fourth International Congress of Zoology" (*Review*), . 221
,, of the United States National Museum" (*Review*), . . 249
Progress of Science, ... 247
,, of a Great Work, . . . 246
Protective Adaptation, ... 89
Protoplasm, ... 166, 183
Protoplasmic Currents, ... 138
Puffin Island, ... 42
,, Excavations on, by Philip J. White, ... 42
Pump Benthos 140
Pycraft on Diastataxy, ... 321

Quarrying, ... 63

Reasoning, Psychology of, . . 225
Recapitulation Doctrine, . . . 82
Record of a Great Work, . . . 384

PAGE

Red Blood Corpuscles, ... 141
Reduction of Nucleus in Plants, . 314
Regeneration in Orthoptera, . . 320
Reid, C., "Origin of the British Flora" (*Review*), . . . 94, 226
Rendle, A. B., "Catalogue of the African Plants collected by Dr. Welwitsch" (*Review*), . . . 364
Ribbed Toads, Expansion of the Empire of, ... 88
River Flow, ... 146
Rock out of Place, ... 315
Rodents, Phylogeny of the, . . 383
Rotifer, A Rare, ... 82
Rust, Phylogeny of, ... 389

St. Kilda, New Mice from, . . 387
Salamander, A new, ... 288
Salt, H. S., "The Logic of Vegetarianism: Essays and Dialogues" (*Review*), ... 365
Sand Dunes, Flora of, ... 10
Sars, G. O., "An Account of the Crustacea of Norway" (*Review*), . 292
Scalpellum vulgare, ... 14
Scaly Squid, ... 4
Scapanorhynchus, ... 440
Scharff, R. F., "The History of the European Fauna" (*Review*), . . 357
Science and Conduct, ... 95
,, a Zoologist on the Principles of, by F. A. Bather, . . 423
,, History of, ... 247
,, Integration in, ... 161
,, Lessons in, ... 365
,, The morning of, ... 247
Scientific Explanations, ... 250
,, Spirit, ... 237, 363
Seals, ... 17
Seasonal Nature-study, ... 145
Sea-urchin, the Production of Parthenogenesis in a, ... 382
Seeds, Dispersal of, ... 94
Self-defence in Plants, ... 3
Sense of Smell, ... 140
Serials, Reviews of, 69, 149, 230, 299, 367, 456
Sexual Dimorphism in Beetles, . . 214
,, ,, Jurassic Nautili, 215
,, Peculiarity, ... 61
Sharks' Tails, ... 215
Sharp, D., "Insects" (*Review*), . 222
Shelley, H. C., "Chats about the Microscope" (*Review*) . . . 227
Sherborn, C. D., "An Index to the Names of Animals described by Linnaeus" (*Review*), 229
,, ,, Lacepède's "Tableaux des Mammifères et des Oiseaux, 1799," . 406
Silurian Rocks, ... 216
Singapore Fauna, ... 130
Sipunculus 141
Sleep, Theory of, A. L. Herrera, . 314
Sloane, T. O'C., "Liquid Air and the Liquefaction of Gases" (*Review*), . 147
Smell in Birds, ... 140
Smith, D. T., "The Philosophy of Memory" (*Review*), ... 146

PAGE

Snail, ... 213
Snails and Plants, ... 3
Snow-worm, ... 286
South African Diamond, the Original Rock of the, by T. G. Bonney, . 173
Sparrows, Elimination in, . . . 441
,, in America, ... 309
Specificity of Cells, ... 291
Spengel, J. W., "Ueber einige Aberrationem von Papilio machaon" (*Review*), ... 366
Sphenodon, ... 87
Spider, Early Rising, ... 13
Spinax niger, ... 288
Spinning at Dawn, ... 13
Sponges, ... 445
Sports, More, ... 392
Squirrels, ... 13
Statistical Method, ... 325
Stick-insect, ... 441
Sting of Hymenoptera, ... 244
Strange Dish, a, ... 60
Sutherland, A., "Origin and Growth of the Moral Instinct" (*Review*), . 442
Swallows, ... 213
Symbiosis, ... 85
Symmetry, some Considerations concerning, by R. J. Anderson, . . 97

Tail, ... 355
Tanganyika, a Jurassic Sea, . . 16
Tayler, J. L., The Scope of Natural Selection, ... 114, 183
Teleology, ... 291
Teratologia, ... 215
Teratology, ... 240
Termites, ... 366
Thomson, J. A., "Outlines of Zoology" (*Review*), ... 144
Thorndike reviewed, ... 1
Thyroid and Thymus of Amphibians, 287
Tides, ... 443
Tierreich, Progress of Das, . . 246
Tilden, W. A., "Short History of the Progress of Scientific Chemistry" (*Review*), ... 147
Tobacco and Insects, ... 170
Tree-frog, breeding in, ... 355
,, Cry, ... 60
Trees in Winter, by P. Q. Keegan, . 399
Triassic Cuttlefish, ... 440
Trilobite from Newfoundland, . . 12
Tyndall, J., "Hours of Exercise in the Alps" (*Review*), ... 298
Typhlomolge Rathbuni, ... 288

PAGE

Urns of Sipunculus, ... 141

Variation, ... 325
,, in Butterflies, ... 366
,, in Chelonia, ... 224
,, Statistics in Zoology by Georg Duncker, . . 325
Variations, An Extension of the Method of treating, with Examples and certain Conclusions, by H. M. Kyle, ... 410
Vine, Cultivation of the, in Essex, . 169
Vipers, Venom of, ... 324
Vitalism, ... 242

Wagner, A. D., "Studien und Skizzen aus Naturwissenschaft und Philosophie" (*Review*), ... 363
Walker, J., "Views on some of the Phenomena of Nature" (*Review*), . 67
Warbles, ... 288
Warren, J. B. L., "The Flora of Cheshire" (*Review*), ... 290
Water Hyacinths, ... 311
Water-plants as Land-winners, . 245
Weather and Conduct, ... 49
Welwitsch's Collection, ... 364
White, J., Excavations on Puffin Island, ... 42
Willey, A., "Zoological Results" (*Review*), ... 224
Wilson, E. B., on Protoplasm, . . 166
Wings of Birds, ... 321
Witchell, C. A., "The Cries and Call-notes of Wild Birds" (*Review*), . 148
Women and the Learned Societies, . 162
Woodhead, S. A., *see* Dawson, . . 347
Wood's Holl Biological Lectures, . 385
Woodward, A. Smith. The Supposed Existing Ground-Sloth of Patagonia 351

Year-Book of the United States Department of Agriculture" (*Review*), ... 359

Zander on the Sting of Hymenoptera, 244
Zehnder, L., "Die Enstehung des Lebens aus Mechanischen Grundlagen entwickelt" (*Review*), . . 227
Zoo at Stuttgart, ... 172
Zoological Congress, ... 221
Zoology in Brazil, ... 8
,, Variation-Statistics in, by G. Duncker 325
Zoos, A Note on, ... 172

VOL. XV., No. 89. JULY 1899.

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

CONTENTS

PAGE

NOTES AND COMMENTS 1

The Animal Mind—The Art of Self-Defence—Trustees of the British Museum—The Scaly Squid—Echinoderms at the British Museum—Accessions to the Natural History Museum—Bryozoa and Bipolarity—More about "Bipolarity"—Natural Science in Australia—The Colouring Matter of Blue Coral—Zoology in Brazil—According to the Fancy of the Speller—Flora of Sand Dunes—Galway Natural History Museum—Botanical Biography—A new found Trilobite from Newfoundland—Mexican and Central American Squirrels—Spinning at Dawn—E pur si muove!—A Complementary Male—Is Fertility Inherited?—Living Fossils.

NOTES ON THE HABITS OF THE NORTHERN FUR SEAL . . . 17
By G. E. H. Barrett-Hamilton.

EXCAVATIONS ON PUFFIN ISLAND 42
By Philip J. White, M.B.

MR. F. W. HEADLEY ON EVOLUTION 46
By R. F. Licorish, M.D.

METEOROLOGY AND ETHICS 49

THE COMPARATIVE CHEMISTRY OF OUR FOREST TREES . . . 53
By P. Q. Keegan, LL.D.

FRESH FACTS 60

SOME NEW BOOKS 62

OBITUARY 70
Rudolf Leuckart.

NEWS 72

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VOL. XV., No. 90. AUGUST 1899.

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

CONTENTS

	PAGE
NOTES AND COMMENTS . . . . . Against the Tide—A Rare Rotifer—Does the Organism Repeat Itself?—Nephrite—The Ordeal by Fire—American Species of Peripatus—Wearing of the Green—Brevis esse laboro, obscuris fio—The Parietal Eye—The Expansion of the Empire of Ribbed Toads—Degrees of Protective Adaptation—An Entomological Exhibition—At Last?—Colours of Northern Monocotyledons—The Proper and Improper View of Heredity—Darwin's Doggedness—Dispersal of Seeds—Reformed Nomenclature!—Science and Conduct.	81
SOME CONSIDERATIONS CONCERNING SYMMETRY . . . . . By Professor R. J. Anderson.	97
THE FLORA OF THE ALPS . . . . . By Professor Alfred W. Bennett, M.A., B.Sc., V.P.R.M.S.	109
THE SCOPE OF NATURAL SELECTION . . . . . By J. Lionel Tayler.	114
STRAY IMPRESSIONS OF THE MARINE INVERTEBRATES OF SINGAPORE AND NEIGHBOURING ISLETS . . . . . By F. P. Bedford, M.A.	130
A THEORY OF SLEEP . . . . . By Professor A. L. Herrera.	134
FRESH FACTS	140
SOME NEW BOOKS	142
OBITUARY . . . . . Sir W. H. Flower, K.C.B.	151
NEWS	154

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VOL. XV., No. 91. SEPTEMBER 1899.

E. H. Turner

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

CONTENTS

	PAGE
NOTES AND COMMENTS . . . . . Integration in Science — Women and the Learned Societies — British Botany—Polemics and a Parasite—Life High and Low—As regards Protoplasm—The Darmstadt Museum—An Annelid from the Devonian—Cultivation of the Vine for Wine in Essex—Did Palaeolithic Man inhabit Scotland?—Insects and Tobacco—Ichthyosaurus at Home—A Note on Zoos.	161
THE ORIGINAL ROCK OF THE SOUTH AFRICAN DIAMOND . . By Professor T. G. Bonney, D.Sc., LL.D., V.P.R.S.	173
THE SCOPE OF NATURAL SELECTION (*continued*) . . . . By J. Lionel Tayler.	183
SUGGESTIONS UPON THE ORIGIN OF THE AUSTRALIAN FLORA . . By Spencer Moore, B.Sc., F.L.S.	198
FRESH FACTS	213
SOME NEW BOOKS	216
OBITUARIES . . . . . Carl Claus.	232
NEWS	233
CORRESPONDENCE	236

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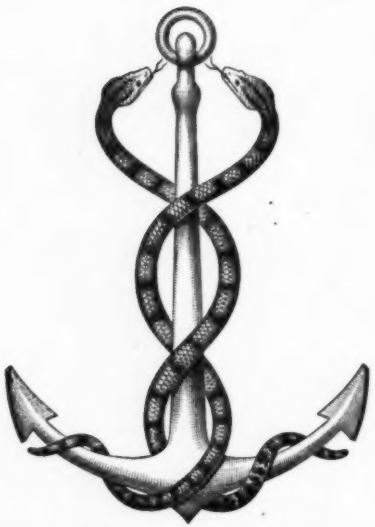

NUNQUAM ALIUD NATURA, ALIUD SAPIENTIA DICIT.

VOL. XV., No. 92. OCTOBER 1899.

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

CONTENTS

PAGE

NOTES AND COMMENTS 237
The Scientific Spirit—More Pleurococcus—Asexual Nuclear Fusions—Inheritance of Malformations—The Nucleolus in Heredity—Inheritance of Longevity—A Verbose Vitalist—Morphology of the Sting in Hymenoptera—Factors in the Growth of Muscle—Water-Plants as Land-Winners—The Progress of a Great Work—The Hopkins Seaside Laboratory—The Morning of Science—Eruption of Mauna Loa—The Poison of Darnel—Coppinia—Scientific Explanations—An Unsolved Problem.

THE INFLUENCE OF THE NERVOUS SYSTEM IN ORGANIC EVOLUTION . 253
By R. F. Licorish, M.D.

THE FAUNA OF THE SOUND 263
Abstracted by F. A. Bather from the Swedish of Dr. Einar Lönnberg

SUGGESTIONS UPON THE ORIGIN OF THE AUSTRALIAN FLORA (*continued*) . 274
By Spencer Moore, B.Sc., F.L.S.

FRESH FACTS 287

SOME NEW BOOKS 289

OBITUARIES 301
John Cordeaux.

NEWS 302

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VOL. XV., No. 93. NOVEMBER 1899.

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

CONTENTS

	PAGE
NOTES AND COMMENTS	309
Disturbing the Balance of Nature—Notes on American Mammals—American Plant-Notes—A Rock out of Place—Beeren Eiland—The Difficulties of the Australian Museum—The *Antarctic* in the Arctic—Regeneration in Orthoptera—Diastataxy—Immunity Acquired before Birth—Eel Poison and Cellular Immunity—Venom of Vipers.	
VARIATION-STATISTICS IN ZOOLOGY	325
By Dr. Georg Duncker.	
THE CEREAL RUST PROBLEM	337
By George Massee, F.L.S.	
PROBLEM OF HONEYCOMB	347
By Charles Dawson, F.G.S., and S. A. Woodhead, B.Sc., F.C.S.	
THE SUPPOSED EXISTING GROUND-SLOTH OF PATAGONIA . .	351
By A. Smith Woodward.	
FRESH FACTS	355
SOME NEW BOOKS	357
OBITUARIES	370
George Dowker.	
CORRESPONDENCE	371
NEWS	373

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VOL. XV., No. 94. DECEMBER 1899.

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

CONTENTS

	PAGE
NOTES AND COMMENTS . . . . . Eliminated—Nature Studies—The Production of Parthenogenesis in a Sea-Urchin—The Record of a Great Work—Floreat Wood's Holl—Asterionella—New Mice from St. Kilda—Alleged New Mice—British Mammals—Phylogeny of the Rodents—Phylogeny of Rust—Ferments in Fungi—More Sports—A Pontifical Plant—Neptuneopsis—A Note on Inheritance—The Cell as a Unit of Organisation—The Biological Corner of a Natural History Museum—Linné's Type Specimens of Fishes—The Molluscan "Liver" so-called—Phylogenetic Senescence—Studies in Plant Morphology.	381
TREES IN WINTER . . . . . By P. Q. Keegan, LL.D.	399
LACEPÈDE'S "TABLEAUX . . . DES MAMMIFÈRES ET DES OISEAUX" . . By C. Davies Sherborn.	406
AN EXTENSION OF THE METHOD OF TREATING VARIATIONS, WITH EXAMPLES AND CERTAIN CONCLUSIONS . . . . By H. M. Kyle, M.A., B.Sc.	410
A ZOOLOGIST ON THE PRINCIPLES OF SCIENCE . . . . By F. A. Bather, M.A.	423
ON THE MULTINUCLEAR CELLS OF SOME GRASSES . . . By Rudolf Beer.	434
FRESH FACTS	440
SOME NEW BOOKS	442
OBITUARIES	461
CORRESPONDENCE	462
NEWS	464
INDEX	471

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